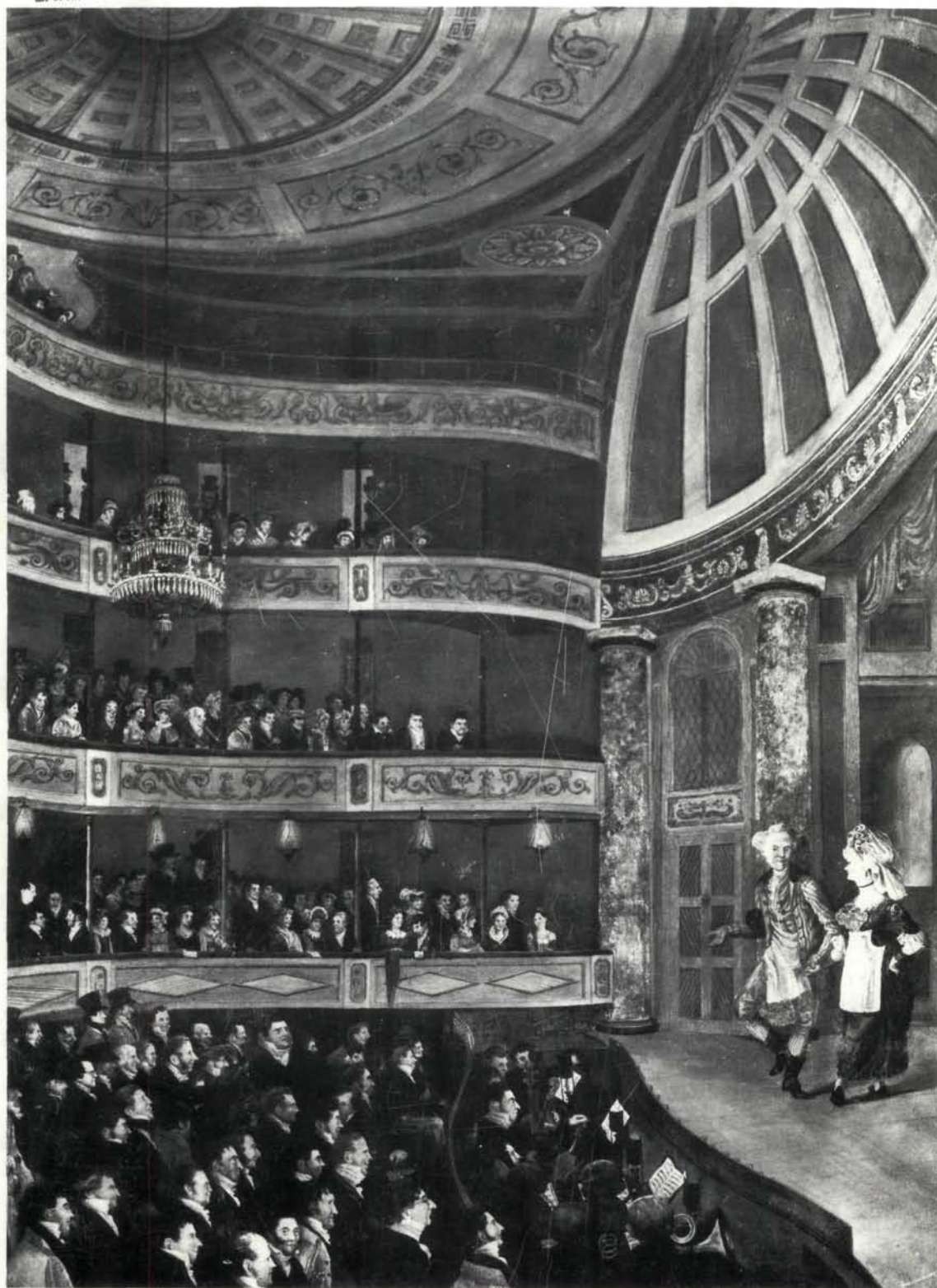


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The Landlord and the Economic Revival of the Middle Ages in Northern Europe, 1000–1250

RONALD G. WITT

ATTEMPTS TO PRESENT a synthetic picture of the economic and social relationships between town and country in the Middle Ages still tend to be dominated by an assumption of class conflict derived either from early liberal or Marxian analyses of the *ancien régime*. The medieval town is usually presented as an antifeudal institution, controlled by a new class of men—the merchants—who because of their lowly social origins and suspicious profession find themselves both despised and feared by the nobility, the dominating class of the agricultural society. Essentially a warrior class whose life is based on an agricultural economy almost completely consumptive in nature, the nobility appears unable to adapt to the expanding use of money. Threatened by the merchants rising from below, the nobility by the thirteenth century is closing its ranks. Economically, however, this attempt at exclusion is of little avail: the manors are breaking up and rights over land and men bargained away for the cash so necessary to pay old debts and purchase new luxuries. So inauspicious is the economic condition of the nobility by 1300 as usually depicted that it is quite marvelous that European revolutionaries of the eighteenth and nineteenth centuries had an *ancien régime* to destroy.

Doubtless the most systematic presentation of the class-conflict thesis for the Middle Ages is to be found in the numerous works of Henri Pirenne. For Pirenne the countryside of the tenth and eleventh centuries had manors everywhere.¹ Small allodial holdings had almost completely disappeared, and the rural population was divided basically into serfs and big landlords who were either nobles or clerics. In his opinion this agricultural society was unable to furnish either leadership or capital for the commercial revival that was just beginning to get under way in these centuries.² The serfs were too downtrodden; the nobility by their very success

I should like to thank Professor Giles Constable of Harvard University, Professor Reinhold Schumann of Boston University, and Professor Charles T. Wood of Dartmouth College for their criticisms and comments.

¹ Henri Pirenne, *Histoire de Belgique* (Brussels, 1900), 1: 99.

² Henri Pirenne, "The Stages in the Social History of Capitalism," *AHR*, 19 (1913–14): 500.

in the feudal agricultural economy was unfit for participation in commercial enterprise. Basic to Pirenne's view of social development, not only of the Middle Ages but in general, is that those who have been successful in one economic system cannot develop their mental powers to attain similar success in a different system. Rather, for him, the merchant class had to be derived from groups of men outside the established order, the outcasts, the runaways, those hostile to the old system, who had both the mental elasticity and mobility to enter commerce.³ These men, of whom St. Goderic of Finchale has become the archetype, had nothing to invest; they relied on their wits and luck; commercial capital was generated by trade itself. In Pirenne's analysis the development of the money economy had for manorialism the disastrous consequences sketched in the opening paragraph.⁴

While not without some reservations about Pirenne's thesis of urban origins,⁵ Marc Bloch made certain conclusions about the development of the medieval economy that tended to support Pirenne's ideas of a town-country, bourgeois-noble conflict. According to Bloch, manorialism on the Continent was in the process of disintegration between the eleventh and thirteenth centuries.⁶ Unlike England the Continent witnessed a secular trend toward the emancipation of serfs and the commutation of labor services, Bloch's obvious assumption being that commutation was intimately related to the landlords' abandonment of direct exploitation of the domain. To deny a diminution in the size of the domain after commutation required an explanation of how the lord could still cultivate his land without forced labor. Slavery was unimportant at this time, and, while hired labor could be utilized in an auxiliary capacity, Bloch thought dependence on paid labor would have led to ruinous consequences, even assuming that surplus manpower had existed in the countryside.

Bloch seems never to have worked out a consistent explanation for this abandonment of direct exploitation by the landlord. On the one hand, he suggested that for economic reasons the landlord decided to restructure the sources of his income. From being an entrepreneur he became a *rentier*. Labor services were commuted against a money payment; domanial lands were extensively rented out; and in order to compensate for a possible loss of revenue, new monetary obligations were imposed on the peasant: "the

³ Perhaps the best summary of Pirenne's thesis of town origins and growth is to be found in Henri Pirenne, Gustave Cohen, and Henri Focillon, *La civilisation occidentale au moyen âge du XI^e au milieu du XV^e siècle* (Paris, 1933), 7-189. These pages were published in English under the title *Economic and Social History of Medieval Europe*, tr. I. E. Clegg (New York, 1937); subsequent citations are to the English translation.

⁴ See especially Pirenne, in Pirenne, Cohen, and Focillon, *Economic and Social History*, 79-86. See also Henri Pirenne, *Medieval Cities, Their Origins and the Revival of Trade* (Princeton, 1925), 171, 223-24, 230-31.

⁵ See, for example, Marc Bloch, "Une synthèse de l'histoire économique médiévale," *Annales: Économies, Sociétés, Civilisations*, 7 (1935): 79-80.

⁶ Marc Bloch, *French Rural History, An Essay on Its Basic Characteristics*, tr. Janet Sondheimer (Berkeley, 1966), 94-101.

master, naturally, sought to recover on one side what he lost on the other.”⁷ To the customary rent, landlords from the twelfth century on began to add new charges based largely on their lordly jurisdiction (the ban or *districtus*, comprising the collection of court fees, royal tailles, and tithes) and on their seigneurial monopolies (the *banalités* on the oven, the mill, and other common uses).

On the other hand, Bloch appears to have suggested that the commutation of labor services was largely a result of the competition for peasants arising from the opening of new areas of settlement in the twelfth and thirteenth centuries.⁸ Not only was land cleared within the older, settled regions, but also on the frontiers, especially in the east. Peasants were attracted to these new areas of settlement by the promise of reduced obligations, and this made it difficult for established landlords to hold their peasants. The example of the town, moreover, “encouraged the peasant masses, and the attraction which the privileged towns might exercise gave the masters cause for concern.” Indeed, in the grip of the communal movement “with its violent hostility to a stratified society,” the town appears to have become a center of “revolutionary ferment.”⁹ These pressures, therefore, forced landlords all over Europe to lower demands and to commute labor services. How the apparently poor bargaining position of the landlord could be reconciled with the imposition of new obligations like the *taille* was never explained.

While Bloch himself did not make any general observations about the relative economic position of the landlords by 1300, his general conclusion that the landlords were becoming *rentiers* by the thirteenth century and his ambiguous explanation of this development lent credence to Pirenne’s emphasis on the deteriorating economic situation of the nobility and on their inability to adapt to an increasing commercialization of agriculture.

Bloch presented a description of the medieval nobility as a social class, moreover, that justified Pirenne’s view of the relationship between the nobility and merchants as one of hostility.¹⁰ Bloch believed that the Carolingian aristocracy had not survived the fall of the Empire. During the turbulent tenth century a man from the lowest ranks of society could gain pre-eminence. The chief talent for such success was skill at arms. Ability in war and wealth in land were the qualities that distinguished the leading families in this age of confusion. By the eleventh century a new concept of nobility was already being defined in terms of the institution of knighthood.¹¹ By the middle of the twelfth century admission to the status of nobility was gained through the ceremony of dubbing into

⁷ Marc Bloch, *Feudal Society*, tr. L. A. Manyon (Chicago, 1961), 253. See also Bloch, *French Rural History*, 95–96.

⁸ Bloch, *Feudal Society*, 276–77.

⁹ *Ibid.*, 355.

¹⁰ *Ibid.*, 283–331.

¹¹ *Ibid.*, 312–27.

knighthood. In the course of the next hundred years what had been a relatively open group became a restricted and exclusive class. For Bloch the nobility was never a completely closed class, but by the last part of the thirteenth century "the door was nevertheless only very slightly ajar."¹² Behind the growing tendency to restriction, therefore, Bloch sensed the reaction of the nobility who feared the penetration of their ranks by the nouveaux riches.

Specialized research over the past thirty years points to the necessity of revising this view of class conflict as characteristic of the relationship between town and country, yet so far little attempt has been made to replace it with an alternative general interpretation.¹³ Indeed, when endeavoring to give a synthetic presentation of the economic and social development of the Middle Ages, contemporary historians, some of whom in their own areas of specialization have reached conclusions suggestive of quite a different view, have fallen back on the old class-conflict thesis. The work of Georges Duby furnishes an interesting illustration of this tendency. One of the most important conclusions of Duby's major work on agricultural history, *L'économie rurale et la vie des campagnes dans l'Occident médiéval* (1962), is that in twelfth- and thirteenth-century agriculture the entrepreneurial position of the noble remained strong, favored as it was by long-term trends in the economy. On the other hand, when attempting to give a synthetic picture of medieval town and country in the second edition of his *L'histoire de la civilisation française* (1962), he resorts to a class-conflict thesis reminiscent of Pirenne and Bloch.¹⁴ In this work the nobility is depicted as on the whole unable to adapt successfully to the expanding money economy: "Indeed, for most of the nobles, the new times are a period of constraint and financial difficulty."¹⁵ Pressed hard by a class of nouveaux riches led by the merchants whose origins were lowly, the "impoverished" aristocracy "after the beginning of the thirteenth century" organizes itself into a caste to resist penetration from below.¹⁶

¹² *Ibid.*, 325.

¹³ Léo Verriest presents a partial refutation of Pirenne's theory of town origins from the standpoint of an analysis of the landlords' position and activities in the medieval economy. *Institutions médiévales* (Mons, 1946), 150. See also A. B. Hibbert, "The Origins of the Medieval Town Patriciate," *Past and Present*, 3 (1953): 15-27.

¹⁴ Georges Duby, *L'économie rurale et la vie des campagnes dans l'Occident médiéval* (Paris, 1962); an English translation by Cynthia Postan has been published under the title *Rural Economy and Country Life in the Medieval West* (Columbia, S. C., 1968). Georges Duby and Robert Mandrou, *L'histoire de la civilisation française* (Paris, 1958; 2d ed., 1962); Mandrou was responsible for the last half of the book; an English translation by J. B. Atkinson was published under the title *A History of French Civilization* (New York, 1964). Because no changes were made in the second French edition regarding the passages quoted in the text I have used the translation from the third printing of the Atkinson translation, published in 1970.

¹⁵ Duby and Mandrou, *History of French Civilization*, 121.

¹⁶ *Ibid.*, 122. While differing from Pirenne in maintaining that the town population "comes primarily from the peasant stock—recently arrived from nearby villages" (p. 73), Duby re-emphasizes Pirenne's view of the humbleness of the early merchants: "The twelfth-century merchant, then, is a vagabond; he takes with him all the commodities he owns" (p. 70). To my knowledge even the very best textbooks on the medieval period now in use in the United States

The objective of the following analysis is to develop the implications of recent, detailed research by Duby and others into a coherent description of the relationship between town and country, merchant and landlord (not necessarily "noble"), a description that fully recognizes the active role played by the landholding classes in the economic revival of the Middle Ages and the success that this group as a whole achieved in adapting to an increasingly commercialized economy. While not denying some degree of conflict both of class and interest, I hope by stressing the largely successful response of the landlords to the "new times" to remove a major explanation for the presumed hostility felt by that group toward the nouveaux riches. In the first section below I characterize the agricultural society of Northern Europe from about 1000 to 1250, and in the second I concentrate on urban development in the same period in the Lowlands, the major area used by Pirenne as the basis for proving his thesis of town-country, merchant-noble antagonism. I have intentionally limited myself in this article to the period from 1000 to 1250 because, as I deal in the second part primarily with the relationship of town and country in the Low Countries, I am concerned with an area that appears to have undergone precocious economic troubles relative to the rest of Europe. In the last half of the thirteenth century, at least, the areas of Flanders and Artois were experiencing a time of troubles.¹⁷ Because economic difficulties affected both town and country, it would be misleading to treat agricultural

describe social categories and relations between town and country in such a way as to suggest class conflict as characteristic of the early evolution of the money economy in Europe. Joseph R. Strayer's most recent revision of Dana C. Munro's *The Middle Ages, 395-1500* (5th ed.; New York, 1970) still conveys the impression that urban development was almost entirely a product of lower-class efforts. Originally from the lowest elements of medieval society (see, for example, discussion 203), the merchant class attained power and wealth by the thirteenth century in spite of the antagonism of the rest of society: "Noble, cleric, and peasant looked upon the townsman with suspicion, and their suspicion was repaid with mocking scorn" (p. 408). On the other hand, the continental nobility are presented as primarily *rentiers* by this century. Living on fixed incomes from their lands in a period of rising prices "it is not surprising that the financial position of the nobility became precarious" (p. 395). As the actual agricultural entrepreneurs in this favorable agricultural market situation, the peasants found their position greatly improved because "the ordinary feudal lord was too careless about financial matters to find ways of depriving them of their increased income" (p. 411). All in all, for Strayer the nobility of the thirteenth century appears to be floundering economically, victims of the wily peasant and shrewd bourgeois financier. Strayer's account, however, deserves praise for its attempt to distinguish between the knights and the nobles in the period prior to 1200 (p. 386). If Robert S. Hoyt, in his *Europe in the Middle Ages* (2d ed.; New York, 1966), omits a specific description of the relationship between rural and urban classes in the twelfth and thirteenth centuries, his opinion of the deterioration in the economic position of the continental nobility in the course of these two centuries is unambiguous and implies a resentful attitude among the nobility toward the peasant and the bourgeois who presumably were draining it of its wealth: "The nobles were caught between fairly fixed incomes and slowly rising prices which resulted in a gradual decline in their relative standard of living. . . . Pressed for money to maintain their more expensive standard of living, or to pay off indebtedness, the nobles often found their only financial solution in selling land, a solution which undercut further the kind of wealth on which the nobility was based" (p. 439).

¹⁷ Eleanora Carus-Wilson, "The Woolen Industry," in *The Cambridge Economic History of Europe*, 2, ed. M. M. Postan and E. E. Rich (Cambridge, 1952), 398-408; Jean Lestocquoy, *Les villes de Flandre et d'Italie* (Paris, 1952), 134.

changes in isolation. Such treatment might give the impression that the town and the money economy were finally having their effect on the financial strength of the landlords, whereas in fact in these same years the prosperity of the commercial and industrial sectors of the economy was being undermined in the older centers also.

THE LONG-TERM ECONOMIC TRENDS of Northern European society from the late tenth to the last half of the thirteenth century were favorable to those groups that controlled land. Whether because of increased security, climatic change, or improvements in food production, the population of Western Europe within this period increased enormously and, despite a noticeable expansion of areas of cultivation, the prices of foodstuffs increased markedly. In England from about 1175 to 1225 average wheat prices appear to have doubled or tripled.¹⁸ The rise, however, was less rapid in the rest of the century. In Picardy between 1150 and 1300 the price of grain and meat rose about 500 per cent, while land prices advanced by 350 per cent.¹⁹

Despite the recognized increase in population and the consequent rise in the prices of food and land in this underdeveloped economy, many economic historians continue to stress the decline in the financial position of the landlords in these centuries. As I have suggested, abandonment of direct exploitation is regarded as one of the most significant indicators of this alleged financial deterioration, largely because direct exploitation was one of the basic aspects of manorialism. The other was labor services, the traditional means for working the lord's lands. One underlying assumption of such an approach to medieval agriculture is that the fate of the landlords is inextricably linked with the fate of manorialism. Since no one can dispute that manorialism at least by the thirteenth century was on the decline, the conclusion is drawn that the landlords were in difficulty.

On the contrary, most recent research on this matter seems to me to indicate, first, that if labor services diminished in the course of the twelfth and thirteenth centuries, direct cultivation of the estate remained vigorous; and, second, that landlords had methods of exploiting their lands other than direct cultivation that were equally profitable. Leasing of domanial lands,

¹⁸ Duby, *L'économie rurale et la vie des campagnes*, 1: 233. See also D. L. Farmer, "Some Price Fluctuations in Angevin England," *Economic History Review*, 2d ser., 9 (1956): 34-43; and his "Some Livestock Price Movements in Thirteenth Century England," *Economic History Review*, 2d ser., 22 (1969): 1-16. Farmer maintains in the latter article that between 1180 and 1210 prices more than doubled (p. 9).

¹⁹ Robert Fossier, *La Terre et les hommes en Picardie* (Paris, 1968), 2: 578. Fossier seems determined to prove, however, that the landlords of Picardy were in trouble in the thirteenth century because of the high cost of labor and operating expenses. He admits the impossibility of giving figures for salaries (p. 575), yet maintains that increased value of land did not keep up with labor costs in the thirteenth century. He speaks of "le prix du sol ne s'élevant guère au cours du XIII^e" (p. 620), an observation that clearly contradicts his graph indicating an increase in price of land between 1190-1200 and 1280-90 of about 300 per cent (p. 581).

for instance, while perhaps a desperate expedient in some cases, should in others be regarded as the result of a business decision to choose a more profitable over a less profitable means of exploiting a basically favorable economic position.

One other fundamental assumption underlying much of the discussion of medieval landholding must be identified at this point. Economic historians tend to assume the persistence of large and medium holdings from the ninth until the twelfth centuries and to discuss the increasing fragmentation of such holdings in the course of the following centuries. The actual situation was doubtless more complicated. In northern Italy and southern France in the period 800–1000 there was a tendency toward fragmentation of holdings that was subsequently reversed.²⁰ Eleventh-century landlords in these areas concentrated their efforts and economic capital on reconstituting and piecing together estates. This increasing awareness of the economic importance of land in a time of demographic rise was further reflected by the marked increase in the percentage of leases in northern and central Italy after 1100 that specified rents in kind rather than in money. The shift from payment in money to one in kind represented in many cases a sizable hiking of the rent.²¹ It is perhaps not coincidental that in the same period monastic establishments in the Low Countries were also in the process of reconstituting and reorganizing their lands. To some degree this process was a product of the religious reforms of these years, but the economic motivations should not be overlooked.²²

The actual evidence usually presented to prove a massive flight from direct cultivation of the domain by the landlord in the twelfth and thirteenth centuries has a built-in bias. The most complete records for the period come from the great Benedictine abbeys and from the archives of princely landlords. These abbeys were among the most traditional and rigid type of ecclesiastical establishments, while the territorial princes from the eleventh century on found themselves in an extraordinarily difficult financial position: the great lay lords were burdened with the problem of meeting fast-rising governmental expenses, and until taxing powers could be developed their principal source of revenue had to be their agricultural holdings. The need for specie and the difficulties inherent in large-scale land management help to explain the appearance of leases on these big holdings from as early as the first part of the twelfth century. It should be no surprise that the first records involving leasing of domain lands in Namur concern the holdings of the count.²³ Even when the great

²⁰ David Herlihy, "The Agrarian Revolution in Southern France and Italy, 801–1000," *Speculum*, 33 (1958): 23–41.

²¹ David Herlihy, "Rural Seigneurie in Italy, 751–1200," *Agricultural History*, 33 (1959): 68.

²² Édouard de Moreau, *Histoire de l'Eglise en Belgique* (Brussels, 1945), 2: 235–36. The attitude of the abbots of Saint-Amand illustrates this clearly. Henri Platelle, *Le temporel de l'abbaye de Saint-Amand des origines à 1340* (Paris, 1962), 122–61.

²³ François Ganshof and Adriaan Verhulst, "Medieval Agrarian Society," in *The Cambridge*

landlords intended to keep control of the exploitation through *villici* they often had difficulty in preventing these officials from usurping rights. The role of the advocates in the deterioration of ecclesiastical patrimonies, moreover, was considerable. The need to reward loyal followers and to obtain allies also diminished the areas of land exploited directly by great lay and spiritual landlords. To make conclusions about the economic abilities of the landholding class as a whole, therefore, on the basis of the evidence from big estates is very misleading.

Even in the case of these estates, however, it would be erroneous to claim that leasing of the domain was the predominant arrangement by 1250. For the Benedictine establishments the very size of the community encouraged the maintenance of a substantial domain.²⁴ At least until 1250, for example, domanial agriculture on the lands of St. Amand near Tournai remained strong.²⁵ The lease became dominant there only in the course of the second half of the century. Besides the lands of the count, few examples of leasing of domanial lands can be found for Namur before 1300.²⁶ After that time the trend toward leasing of large lay and ecclesiastical estates became general in Northern Europe, but in the fourteenth century this trend should be seen against the altered circumstances in both rural and urban sectors of the economy.²⁷

While medium landholders, lay and ecclesiastical, were liable to some of the same difficulties as the great lords, it would appear that the vast body of these lords retained immediate control of production throughout the period 1000–1250.²⁸ As agriculture became progressively commercialized from the eleventh century on, there was naturally a greater instability of landed fortunes. Some of the landlords in this group went into debt and lost their estates. Manors were divided frequently because of the laws of succession. Domanial lands were often given as offerings to the Church.

Economic History of Europe, 1, ed. M. M. Postan (2d ed.; Cambridge, 1966): 322; Léopold Genicot, *L'économie rurale namuroise au bas moyen âge* (Louvain, 1943), 1: 107.

²⁴ M. M. Postan, "Medieval Agrarian Society in Its Prime: England," in *Cambridge Economic History*, 1: 577–78.

²⁵ Platelle, *Le temporel*, 264, 272.

²⁶ Genicot, *L'économie rurale namuroise*, 1: 106–10.

²⁷ In this century even middling landlords were giving up direct cultivation of their domains. Duby, *L'économie rurale et la vie des campagnes*, 2: 579–93; and his "La grande domaine de la fin du moyen âge en France," in *Première conférence internationale d'histoire économique* (Paris, 1960), 1: 333–42.

²⁸ Genicot accepts the importance of domanial cultivation for smaller landlords while tending in his overall conclusion to exaggerate—given the chronology of his evidence—the extent of leasing in the thirteenth century among the bigger landholders. *L'économie rurale namuroise*, 1: 120. For England Postan emphasizes "bouyancy" for domanial cultivation. "Medieval Agrarian Society in Its Prime," 581. Duby convincingly argues for the resiliency of direct exploitation of the domain by the middling landlord throughout the century for France. *L'économie rurale et la vie des campagnes*, 2: 507–12. Fossier stresses the importance of direct cultivation for the landlords of Picardy. *La Terre*, 2: 634. Ganshof and Verhulst maintain in the case of Germany that "the increase in the landed wealth of the nobility, particularly the estates of medium size, continued throughout the thirteenth century." "Medieval Agrarian Society," 304. They curiously do not specify how the nobility profited from the added land.

Some were conveyed to vassals in the form of benefices. But it is fair to say that the texts indicating an extension of domanial cultivation are almost as numerous as those showing its diminution.²⁹ The new orders of monks that rose in the twelfth century placed an emphasis on direct cultivation by the monks themselves or by their *conversi* as opposed to the more seigneurial form of the older monasticism. In the clearing of new lands around the older estates or in the foundation of completely new settlements the landlord frequently obtained an important part of the land for his own use. Peasant families often died out or moved away, and this permitted the landlord to incorporate the tenures back into the domain. Not content with the exploitation of their own domains, moreover, prosperous landlords like Reginald of Alherimont, cleric of the archdiocese of Rouen, probably constituted the principal market for land offered for lease by the great lords.³⁰

Usurpation was a frequent source of increased holdings. Perhaps most of the land lost by great monasteries or surreptitiously subtracted from the holdings of a great territorial prince came into the hands of this land-hungry group of middling landholders. In their position as advocates of ecclesiastical institutions or as *villici* for great spiritual and temporal lords, men of this class were ideally placed for such activities. It required the presence of Bernard of Clairvaux to protect the monks of St. Vaast-en-Cambrésis from the exactions of their own *villicus*, while in the same period at Gembloux the *villicus* of the monastery, an important man locally, reputedly used his position to mulct the brothers.³¹

Consequently direct exploitation remained vigorous in the twelfth and thirteenth centuries. How then explain the commutation of labor services, which Bloch assumed to be the sign that the landlords were abandoning direct cultivation? Certainly in some cases he was correct in making this assumption, but, generally speaking, the commutation of labor services, which signaled the decline of manorialism, did not mean the decline of the lord's control of agricultural production. The commutations of the twelfth century on the Continent, as Bloch himself observed, could be convenient for both parties, peasant and lord alike.³² But contrary to his interpretation it seems that many landlords probably approved of the conversion of labor services into money payments primarily because it permitted them to exploit their fields more efficiently. Forced labor was notoriously inefficient, and with the money made available through commutation the landlord could hire much of the work done. Only half the

²⁹ The observation is made by Duby, *L'économie rurale et la vie des campagnes*, 2: 418. See also Ganshof and Verhulst, "Medieval Agrarian Society," 313.

³⁰ See document in *Regestrum Visitationum Archiepiscopi Rhotomagensis*, ed. Théodose Bonnin (Rouen, 1852), 769-71; also included as documents 138 and 139 in Duby, *L'économie rurale et la vie des campagnes*, 2: 729-31.

³¹ Fossier, *La Terre*, 2: 563; Genicot, *L'économie rurale namuroise*, 2: 73. Fossier stresses the relatively high social standing of the *villici* in Picardy.

³² See pp. 966-67 above.

money collected from the tax that the peasants agreed to pay in order to be exempted from work in the abbey's vineyards was needed by the monks of Cluny around 1150 for the salaries of the laborers hired in their place.³³ Besides this, in the course of the eleventh and twelfth centuries tools and modes of cultivation had become more efficient, and less labor than before was required to produce the same quantity. In any case, the landlord often kept the labor services of his tenants for the busiest seasons of the year like planting and harvesting. The rest of the year he could do with a few regular hired men. What made this solution to the problem of labor all the more attractive for the landlord was that in these centuries external and internal colonization had probably been able to relieve only in part the pressure of population increase, and in the course of the thirteenth century population was piling up in many areas. This availability of manpower would have made for lower wages and for greater profits for the landlord.

Who were these hired laborers? Most of the tenants on the estates had tenures that required only the part-time work of the family. If the father of a household was not available for hire, there was almost always a younger son who needed regular work. The population of the estate itself, consequently, was able to satisfy most of the domain's requirements for labor.

The English agricultural world of the Middle Ages, like that across the Channel, increasingly appears to have witnessed a good deal more commutation of labor services than Bloch realized. What were listed as labor services on the Hundred Rolls in the thirteenth century and have been regarded as such by most historians were in fact actually commuted by the peasants.³⁴ The insistence on performance of "labor services" was one way for the landlords to force money out of their dependents in a period when tenures were in short supply.

As I have already suggested, in an increasingly complex economy like that of medieval Europe in the twelfth and thirteenth centuries direct exploitation of the domain was not the only way for a landlord to maximize his profits. The great domain of the Cistercian nuns of the Paraclete in twelfth-century Picardy seems a prosperous enterprise with almost all of its lands rented. Although on a part of the land given out in this manner the nuns received a fixed amount of wheat, on most of it the landlord's share was half the harvest.³⁵ In Lower Saxony the landlords in the same

³³ Georges Duby, "Problèmes d'économie seigneuriale dans la France du XII^e siècle," in *Probleme des 12. Jahrhunderts* (Constance, 1968), 161-67.

³⁴ M. M. Postan originally maintained that in the thirteenth century in England there was a reversal of the trend toward commutation of labor services. "The Chronology of Labor Services," *Transactions of the Royal Historical Society*, 4th ser., 20 (1937): 169-93. Bryce Lyon questions this reversal. "Encore le probleme de la chronologie des corvées," *Le Moyen Age*, 4th ser., 18 (1963): 615-30. And Postan himself seems to have modified his views about the phenomenon in recent years. See his review of Duby's *L'économie rurale et la vie des campagnes* in *Economic History Review*, 2d ser., 16 (1963): 197; and his discussion in "Medieval Agrarian Society in Its Prime," 607-08.

³⁵ Fossier, *La Terre*, 2: 630.

period found it profitable to emancipate their customary tenants, expel them from the land, and, regrouping the holdings into larger units, give these units out to the more prosperous peasants on the basis of a lease.³⁶ Moreover, part of the reason behind the belief in a deterioration in the position of the landlords stems from the assumption that, since many of the lord's rents were fixed monetary amounts, the landlord saw his income severely diminished as money depreciated over the centuries. This assumption overlooks the fact that the predominant method of payment to the lord throughout these centuries was probably still in kind.³⁷ While dues were sometimes represented in the manor rolls in money amounts, the landlord often in fact had the choice of receiving these dues in goods rather than in specie. Similarly English landlords partially solved the problem of fixed obligations by availing themselves of the occasion furnished by a change of tenant family to exact an entry fee whose incidence gradually increased in the course of the thirteenth century.³⁸ The landlords of the Paris region took the same opportunity to add an *incrementum census* to the traditional *census* owed for a holding.³⁹

Neglecting the apparent inconsistency in Bloch's analysis—which stresses the establishment of new types of exactions by landlords from the twelfth century on while at the same time suggesting that lords were forced to lighten impositions in order to retain their peasants—should we not follow him in regarding the landlords' decision to rely on the ban and the various *banalités* as a conscious attempt to substitute an alternative and at least equally profitable source of income for traditional dues? Very possibly this sort of business decision was made when in the course of the thirteenth century countless landlords of southern Germany, apparently willing to permit the division and redivision of tenures and domain lands by the peasantry, decided to focus on their power of the ban as the principal source of income from their old and new tenants.⁴⁰ Did not the effectiveness of this means of revenue rest on a long-run economic tendency in favor of the landlord, on the reluctance of tenants to abandon a precious holding in a time of growing land scarcity?

The major source of surplus food in medieval society came not from the small peasants but from larger holdings. As the commercial revival progressed, the lord through his possession of woods and waste was in a position to profit from the increasing demand for wool and leather. In

³⁶ Charles Édmond Perrin, *La seigneurie rurale en France et en Allemagne du début du IX^e à la fin du XII^e siècle* (Paris, 1953), 3: 286–87.

³⁷ Duby, *L'économie rurale et la vie des campagnes*, 2: 439.

³⁸ M. M. Postan and J. Z. Titow, "Heriots and Prices on Winchester Manors," *Economic History Review*, 2d ser., 11 (1959): 392–411.

³⁹ F. Olivier Martin, *Histoire de la coutume de la Prévôté et Vicomté de Paris* (Paris, 1922), 1: 382. If the landlord maintained the old exactions from the established tenures, when he cut out new tenures from the domain he might set the rents for these new holdings much higher and with a certain flexibility. Léo Verriest, ed., introd. to *Le polyptyque illustré dit "Viel Rentier" de messire Jehan de Pamele-Audenarde (vers 1275)* (Brussels, 1950), 73.

⁴⁰ Perrin, *La seigneurie rurale*, 3: 304–05.

Picardy, Flanders, Namur, and Liège, to speak only of the area north of the Somme, woad and madder were agricultural products vital for the dyeing industry.⁴¹ The privileges of the landlord were also frequently of great advantage in competing for the market of agricultural production. When the lord of Ferrières-en-Gâtinais every year put his wine harvest on the market, the taverners who normally sold wholesale were forced to shut their doors.⁴² With rights to carrying services rendered by manorial dependents, moreover, the landlord not only brought his own goods to market but could handle the sale of the excess produce of his peasantry as well.

By modern standards investment on a medium-sized or large agricultural exploitation in the Middle Ages was very low.⁴³ Nevertheless the hundreds of *villes neuves* scattered over the face of Western Europe bear witness to the existence of important capital development by the landlords. While it is doubtless true that some of these settlements were products of the efforts of the peasants themselves, who accepted the invitation of a territorial prince or an important ecclesiastical institution to colonize an area, the greater number of these pioneering developments were directed by the landlords. How many depressed peasants, supposedly those who would be seeking for a better way of life, would have been able to put aside enough food from their daily requirements in order to sustain themselves for the year or more that lay between the departure from their old home and the first harvest from the newly cleared land in the wilderness?

While some of the internal colonization was a part-time affair in the neighborhood of the established manors and could be done over a long period without setting aside a special food reserve, for the more ambitious undertakings immense amounts of capital were required. Frequently extensive drainage projects had to be built in order to render the land fit for use. New settlers were often promised tools, beasts, and seed corn by the landlord. Great landlords frequently employed *locatores* to supervise the settlement. These agents were to a significant degree members of landholding families themselves. Smaller landowners who could not sustain the expenses of opening new lands individually would sometimes group their resources for these undertakings, as did the three knightly possessors of the forest of Marigny who in 1275 agreed to exploit the woods jointly.⁴⁴

⁴¹ André Joris, "Les moulins à guède dans le comté de Namur pendant la seconde moitié du XIII^e siècle," *Le Moyen Age*, 4th ser., 14 (1959): 253-78; Fossier, *La Terre*, 2: 423; Verriest, *Institutions médiévales*, 148-49.

⁴² Duby, *L'économie rurale et la vie des campagnes*, 1: 229.

⁴³ Postan, "Medieval Agrarian Society in Its Prime," 583. Rodney Hilton estimates agricultural investment on medieval estates not to have exceeded five per cent of income per annum in the thirteenth century. "Rent and Capital Formation in Feudal Society," in *Second International Conference of Economic History* (Paris, 1965), 2: 53.

⁴⁴ Doc. 46 in Duby, *L'économie rurale et la vie des campagnes*, 1: 331-32. See also Verriest, *Institutions médiévales*, 1: 130-31; and Ganshof and Verhulst, "Medieval Agrarian Society," 295. For some interesting observations on land clearance and reclamation on the properties belonging to St. Bavon see Adriaan Verhulst, *De Sint-Baafsabdij te Gent en Haar Grondbezit*

It seems, therefore, historically inaccurate to treat the landholding classes of the Middle Ages as passively reacting to economic trends over which they had no control and against which they struggled angrily and desperately in an attempt to shore up the dilapidated edifice of the manorial economy. Rather, the documented responses of landholding groups as a whole reveal the ability of the landholding classes to make intelligent business decisions within an economic context that basically favored their position. Their economic power was not tied to the survival of manorial production—that is, to the direct exploitation of the domain by means of forced labor services rendered by dependent tenants. While cultivation of the domain principally by salaried labor under the control of the landlord continued to be extremely important until the end of the thirteenth century, nevertheless in terms of the economic situation there were many other ways in which landlords could profit from their holdings and control over men. While we know that many lost everything or lived in genteel poverty, as a class the landlords of the eleventh to the thirteenth centuries appear to have been an active, dynamic force in the economy and not its victims. The obvious potentiality of the countryside under the direction of the landlords for generating capital and the willingness of the landlords to use their opportunities have important implications for analyzing the genesis of the urban economy.

THE INCREASING PROSPERITY of the agricultural economy in the course of these centuries depended in no small degree on the growth of urban markets and the intensity of long-distance trade. To what extent were the landholders of the countryside involved in the development of the town, which was unquestionably the *motus animi* of the commercial revival over the long run? To what degree, while not hostile and threatened as Pirenne would have it, were they merely profiting from a movement initiated by a group of social outcasts or members of the lower classes in general?

It is widely recognized that the relationship between town and country, landholder and commercial development, was a close one for the great urban agglomerations of Italy. While commercial life in ninth- and tenth-century Italy was certainly not intense, it never disappeared completely, and there was a certain continuity in commercial activity and probably in merchant groups through the centuries. The Italian landlords, moreover, resided for a good part of the year in the city and were not reluctant to

(VII–XIV eeuw) (Brussels, 1958) French résumé on pp. 509–19). In his well-known "Medieval Real Estate Developments and Freedom" (*AHR*, 63 [1957–58]: 47–61) Bryce Lyon appears to exaggerate the extent to which the peasants involved in early land reclamation projects were free men. See also Adriaan Verhulst, "Landgemeinde in Seeflandern," in *Die Anfänge der Landgemeinde und Ihr Wesen* (Constance, 1964), 1: 451–53; and J. M. van Winter, "Landgemeinde in der Holländisch-Utrechtschen Tiefebene," in *ibid.*, 1: 444.

participate both in business investment and directly in trade itself.⁴⁵ Particularly in the coastal cities *domini* were attracted to commerce in situations where it was easy to combine piracy with trading.

For inland cities, however, this generalization about the role of families of the upper classes in the initial development of the urban economy should be made with a degree of caution. At least in the case of Florence it now appears that many big merchant families, designated as *magnati* and heretofore considered to have stemmed from the feudal nobility, were in all probability newly rich.⁴⁶ Their social position came from early good fortune in trade. Yet even in the case of these "new families" land provided much of the original capital on which the later fortune from commerce was based.⁴⁷

The interest of Pirenne and his immediate followers like Georges Espinas and Hans van Werveke has been focused primarily on the Lowlands area north of the Somme. In his concise summary of the development of towns published in 1963 but actually written just prior to the Second World War, van Werveke re-emphasizes the fundamental position of Pirenne and his group that, if in Italy land was important in capital formation, in the North the merchants were too humble to draw upon such resources, and that landed property furnished capital for commerce in urban areas in the North only at a later stage.⁴⁸ Reviewing the extensive bibliography of studies on urban and merchant origins published in recent decades, Bryce Lyon in 1960 felt justified in observing that "on the continent, especially between the Loire and the Rhine, Pirenne's theory holds a central position which shows no signs of weakening."⁴⁹

Unfortunately these Lowland areas are poorer than Italy in documentation for urban centers before the thirteenth century, and accordingly a great deal of latitude exists for doctrinal interpretation of the scattered and often ambiguous evidence.⁵⁰ The matter of merchant origins is hampered particularly by the frequent absence of surnames or place designations in conjunction with the names of individuals found in the documents. It would be important to know, for example, if that Lambertus, *ministerialis* of the Prince Bishop of Liège and brother of the Abbot of Saint-Airy

⁴⁵ Yves Renouard, *Les hommes d'affaires italiens du moyen âge* (Paris, 1949), 46-47, 62.

⁴⁶ Enrico Fiumi, "La fioritura e decadenza dell'economia fiorentina," *Archivio Storico Italiano*, 117 (1959): 432-40.

⁴⁷ This is one of the basic conclusions of Johann Plesner, *L'emigration de la campagne à la ville libre de Florence au XIII^e siècle* (Copenhagen, 1934).

⁴⁸ Hans Van Werveke, "The Rise of the Towns," in *The Cambridge Economic History of Europe*, 3, ed. M. M. Postan, E. E. Rich, and Edward Miller (Cambridge, 1963): 3-41.

⁴⁹ Bryce Lyon, "L'oeuvre de Henri Pirenne après vingt-cinq ans," *Le Moyen Age*, 4th ser., 15 (1960): 468.

⁵⁰ Jan A. van Houtte, "Gesellschaftliche Schichten in den Städten der Niederlande," in *Untersuchungen zur Gesellschaftlichen Struktur der Mittelalterlichen Städte in Europa* (Constance, 1966), 262-63; Franz Petri, "Die Anfänge des mittelalterlichen Städtewesens in den Niederlanden und dem angrenzenden Frankreich," in *Studien zu den Anfängen des Europäischen Städtewesens* (Constance, 1958), 229.

(1084–1106) who served as the bishop's *provisor publici juris et judex* at Huy, was identical with Lambertus Hoyensis, the first man identified as a merchant of that city (1103).⁵¹

In retrospect Pirenne tended to underestimate the extent of commercial life in the Carolingian period, particularly in agricultural products like wine and industrial goods like textiles.⁵² With historians like Bloch he also exaggerated the amount of destruction occasioned by the Viking incursions in the North Sea area. The supposed destruction of Tournai is based on the unreliable *Historiae Tornacenses* written in the second half of the twelfth century.⁵³ At Quentovic, Arras, St.-Omer, Dinant, Namur, and Huy, moreover, there is no evidence of rupture in commercial life between the eighth and the tenth centuries.⁵⁴ Only at Ghent and Valenciennes does a break in the period of the invasions seem to occur: thirty years for the former and twenty-five for the latter. But in each case "il n'est pas prouvé." Monks heavy with abbey treasures were probably among the first to flee the advent of the Vikings and the last to return, and the period in which monks like those of St. Bavon of Ghent were in exile is not necessarily one in which the *portus* of the settlement was deserted.⁵⁵ In any case, for this area north of the Somme as a whole there is little reason to postulate a break between the merchant class of the Carolingian towns and that of the tenth century.

The nature of the social milieu in which initial urban development occurred in the tenth and eleventh centuries also requires a redefinition. In order not to prejudice this analysis, in discussing the landholding classes of the countryside I have been careful to avoid using the word "noble" to apply to the landlords. There is little question that of the lay landlords the very greatest, like the counts and territorial princes, throughout the period deserve the appellation. Many of these belonged to families that could be traced back to the Carolingian aristocracy of the ninth century and before. Nor was this resiliency peculiar to the very upper strata of society: families of the second order, those that made up the leading adherents of the various territorial princes, often stemmed from the *vassi dominici* of the Carolingian emperors who in the period of imperial dis-

⁵¹ André Joris, *La ville de Huy au moyen âge* (Paris, 1959), 105, 361.

⁵² Perhaps the best general discussion of the extent of this trade is found in Robert Latouche, *The Birth of Western Economy*, tr. E. M. Wilkinson (London, 1961), 235–67. Latouche also is very critical of the monolithic nature of Pirenne's explanation for the development of towns. See also Robert S. Lopez, "Le città dell'Europa post-carolingia," in *I problemi comuni dell'Europa post-carolingia* (Spoleto, 1955), 559–61, 566–67; and Boris A. Rybakow, "Der Handel und die Handelsstrassen," in B. D. Grekow and M. L. Artamonow, eds., *Geschichte der Kultur der Alten Rus* (Berlin, 1959), 1: 316.

⁵³ Albert d'Haenens, *Les invasions normandes en Belgique au IX^e siècle* (Louvain, 1907), 270–75.

⁵⁴ *Ibid.*, 158–59.

⁵⁵ *Ibid.*, 48–49, 141–42. The arguments of François Blockmans ("Les deux 'Portus' successifs de Gand," *Revue du Nord*, 26 [1943]: 13–15) and Hans van Werveke (*Gand, esquisse d'histoire sociale* [Brussels, 1946], 16) regarding the destruction of the abbey are not conclusive for the *portus*.

integration had shifted their allegiance to the new lordships formed out of the wreckage. Although perhaps an exceptional example, a study of leading families of Touraine from the ninth to the twelfth centuries has recently shown that the social composition and stability of the group over this period remained unchanged [*ändert sich . . . überhaupt nicht*].⁵⁶ Accordingly, Bloch's conception of a discontinuity between the Carolingian aristocracy of blood and the nobility of the eleventh century is no longer tenable. Hereditary in its status, proud of its long traditions, powerful politically and economically, this eleventh-century nobility, moreover, was to remain a fairly narrow group at the top of the social ladder down to the twelfth century in certain regions and into the thirteenth or even later in the lands north of the Somme.

Below this class of nobles lay a much larger, more diversified group of landholders, who, generally speaking, controlled the middling estates. The more important *ministeriales* who served the temporal or spiritual lords as knights or domanial and aulic officials derived their income to varying degrees from agricultural possessions both allodial and feudal.⁵⁷ *Censuales* in the *familia* of the great abbeys might also hold sizable acreage like those prosperous *possessores*, the men of St. Marie at Tournai.⁵⁸ The great diversity of social origins of this particular class, which included members of the nobility, made for a great disparity in wealth among its members.⁵⁹ Although it is almost impossible to prove, it is also not unlikely that some of those holding middling estates were completely without any ties of dependence.

As regional studies are published it becomes increasingly apparent that the area of "classic feudalism" grows smaller and smaller. In regions of France and the Low Countries as disparate as Mâcon, Picardy, the Midi,

⁵⁶ Karl F. Werner, "Untersuchungen zur Frühzeit des französischen Fürstentums, 9.-10. Jahrhundert," *Die Welt als Geschichte*, 18 (1958): 256-89; 19 (1959): 146-93; 20 (1960): 87-119; Georges Duby, "Une enquête à poursuivre: la noblesse dans la France médiévale," *Revue historique*, 226 (1961): 1-22; Léopold Genicot, "La noblesse au moyen âge dans l'ancienne 'Francie,'" *Annales: Économies, Sociétés, Civilisations*, 17 (1962): 1-22. Werner's judgment on the nobility of Touraine is found in "Untersuchungen zur Frühzeit," 19 (1959): 185.

⁵⁷ The frequent donations of allodial land to ecclesiastical institutions by *ministeriales*, both serf and free, suggest that servile status presented no bar to such holdings. François Ganshof's explanation of the allodial courts as not concerned with allodial property in a true sense (*Étude sur les ministeriales en Flandre et en Lotharingie* [Brussels, 1926], 379-414) has recently been refuted by Micheline Soenen in "A propos de 'ministeriales' brabançons propriétaires d'alleux aux XII^e et XIII^e siècles," in Georges Despy, Maurice Arnold, and Mina Martens, eds., *Hommage au Professor Paul Bonenfant* (Brussels, 1965), 139-49. See also Marc Bloch, "Un problème d'histoire comparée: la ministérialité en France et en Allemagne," *Revue historique de droit français et étranger*, 7 (1928): 62-63. On the free and serf status of *ministeriales* see Paul Bonenfant and Georges Despy, "La noblesse en Brabant aux XII^e et XIII^e siècles," *Le Moyen Âge*, 4th ser., 13 (1958): 52; Guillaume Des Marez, "Note sur la ministérialité," *Bulletins de l'Académie royale de Belgique, Classe des Lettres*, 5th ser., 10 (1924): 73-82; Léo Verriest, *Noblesse, chevalerie, lignages* (Brussels, 1959), 27; and Genicot, *L'économie rurale namuroise*, 2: 65-77.

⁵⁸ Paul Rolland, *Les origines de la commune de Tournai* (Brussels, 1931), 124-25; Perrin, *La seigneurie rurale*, 2: 211.

⁵⁹ Verriest, *Institutions médiévales*, 187-91; Perrin, *La seigneurie rurale*, 2: 211.

and Namur, by the mid-eleventh century the allod still constituted the principal form of property.⁶⁰ Especially relevant for the northern area of France and for the Low Countries is a recent investigation regarding feudalism in Picardy. In this province the association of a fief with vassalage did not become frequent until the end of the twelfth century,⁶¹ and throughout the thirteenth century only a small proportion of land in the province was held as benefice. Léo Verriest's combative pages criticizing Pirenne's assumption that between the eighth and eleventh centuries allodial land not only was of minor importance in the Low Countries but that what remained of it was firmly in the hands of the aristocracy can be read today with a good deal more sympathy than when it was written twenty-five years ago.⁶²

The significance of allodial holdings in the medieval rural landscape, therefore, meant that real estate was more mobile in the hands of more people and consequently had a greater commercial significance than many historians with their vision of a massively feudalized countryside realized. Although the transfer of allods, whether by gift or sale, frequently required the assent of relatives, especially in the period prior to the thirteenth century, the mobility of allodial possessions was nevertheless much greater than that of lands held on feudal tenure and burdened with all manner of obligations. Above all, division, exchange, or sale of parts of allods was simpler in procedure than in the case of benefices where the overlord usually had an interest in seeing that the benefice was adequate to the performance of the services owed.

In the eleventh century this middle group of landlords was, in contradistinction to the nobility, extremely amorphous. In this society military prowess was highly esteemed, but the prestige enjoyed by the *miles* was qualified by the servile origins of many who exercised the profession of arms. The ceremonies and trappings of chivalry were not as yet codified and identical with the way of life of the nobility.⁶³ There was no social bar to intermarriage between families of *milites* and families of *ministeriales* with aulic or domanial functions. Younger sons of knights in the *familia*

⁶⁰ Georges Duby, *La société aux XI^e et XII^e siècles dans la région mâconnaise* (Paris, 1959), 292; Fossier, *La Terre*, 2: 547; Genicot, *L'économie rurale namuroise*, 1: 69; Archibald Lewis, *The Development of Southern French and Catalan Society, 718-1050* (Austin, 1965), 383. Both Fossier and Lewis produce spectacular figures on the percentage of allodial land in their respective areas. Because their figures are based largely on donations to ecclesiastical institutions we might expect, however, that allods, essentially more mobile than benefices, would predominate in these acts.

⁶¹ Fossier, *La Terre*, 2: 546.

⁶² Verriest, *Institutions médiévales*, 35-36.

⁶³ Édouard Poncelet cogently remarks: "Au XII^e siècle, la chevalerie qui, jusque là, n'avait été qu'une profession, devint, en même temps qu'un corps d'élite, un ordre soumis à des règles sévères relatives à l'honneur, à la courtoisie, à la parfaite loyauté." *Oeuvres de Jacques de Hemricourt*, ed. Camille de Borman, Alphonse Bayot, and Édouard Poncelet (Brussels, 1931), 3: clxx. In the area of Liège only after the end of the thirteenth century were the knights assimilated into the nobility (p. xcvi). For evolution of chivalric life style, see Bloch, *Feudal Society*, 312-19; and Charles T. Wood, *The Quest for Eternity* (New York, 1971), 119-38.

of a lord, moreover, while still remaining in that lord's dependency, probably had a relatively open choice of career: the benefice and the charge of *miles* would have devolved on the eldest son.⁶⁴

At the very inception of the economic revival in the tenth and eleventh centuries many of these *milites* were resident in the towns as castle guards along with other *ministeriales* who served as judges, *monetarii*, and *vicarii*. The *civis* of Bruges who was brother-in-law of one of the knights besieged in the town's fortress in March 1127, or the other who was kin to Lambert Archei, vassal of the count of Flanders, were doubtless not untypical of a social situation where class consciousness at least at this level was not yet highly developed.⁶⁵

The genealogy of the Huquedieu, one of the earliest known patrician families of Arras, illustrates well the adaptability of men belonging to the "established" society. Designated as "a man of St. Vaast," Sawalon Huquedieu, whose father Henry had also probably been a man of the abbey, was an important property owner in the town in the first half of the twelfth century. One of his sons, Sawalon, is designated as *miles* and also *officialis* of the count of Flanders. This second Sawalon and his brothers, whose lives belong to the last half of the twelfth century, were important holders of urban property. The thirteenth-century descendants of the family were leaders in the cloth trade that dominated the commerce of the city.⁶⁶

The *familia* of the bishop of Huy seems to have been the leading element in the early patriciate of that city, which developed commercially in the eleventh century. We can only guess at the economic activities of Dodon, *dispensator episcopalis mense*, who in 1106 was listed as *villicus* and counted among the burghers. The lending activities of the twelfth-century *cellerarius* of the bishop are more certain, as are those of his son who gave money to merchants "so that he might share in the abundant profit of merchants, just as many laymen of quality were accustomed to do." The grandson of the *cellerarius* followed the family lending tradition.⁶⁷

The inventory made in 1145 of the property of Nicolas, *civis strenuus* of Amiens, son of Mainier, *monetarius*, clearly illustrates the origins of wealth of one of the early patrician families of that episcopal city.⁶⁸ Possessed of rural lands with *hospites* as tenants and a house near the Somme with an income derived from rights to collect port fees from merchants using his docks, Nicolas was evidently one of the leading men of the town.

⁶⁴ Poncelet, *Oeuvres de Hemricourt*, 3: cxxx.

⁶⁵ Galbert de Bruges, *Histoire du meurtre de Charles le Bon*, ed. Henri Pirenne (Paris, 1891), 78, 96.

⁶⁶ Jean Lestocquoy, *Patriciens du Moyen-Age: Les dynasties bourgeoises d'Arras du XI^e au XV^e siècle* (Arras, 1945), 85-91.

⁶⁷ Joris, *La ville de Huy*, 359, 361.

⁶⁸ Michel Mollat, *Les villes et la civilisation urbaine entre la Meuse et la Seine du début du XI^e siècle au début du XIV^e* (Paris, 1960), 1: 49-50. For the economic role of the *monetarii* see Robert S. Lopez, "An Aristocracy of Money in the Early Middle Ages," *Speculum*, 28 (1953): 1-44.

The document mentions a three-story warehouse with a cellar projected for the docksite. Very likely Nicolas inherited part of this fortune from his father, Mainier, a minter and in all probability *ministerialis* of the bishop. The failure of Nicolas to call himself *monetarius* might stem from his being a younger son in the family. The instance of Nicolas and Mainier recalls the four *monetarii* of Dinant who were entrusted by the count of Namur with the governing of the town at least from the eleventh century into the thirteenth and whose progeny melted into the class of the *burgenses* of this important industrial and trading center.⁶⁹

Middling landholders of the countryside could easily be concealed behind the titles *negotiatores* or *mercatores*. The merchant of Tournai, Otherlard, who had been described in the first *vita Macharii* (1014) as loading his ship with wool at the *portus* of the city on October 1, 1013, for a voyage to the market at Ghent, was identified by the second *vita* (1067) as one of the *possessores* of Tournai: "As is the custom of landowners [*possessoribus*] . . . he loaded his ship with wool, and, with other merchants [*ceteris mercatoribus*] who brought their goods to this market from all over, he sailed to Ghent."⁷⁰ In a period when the commercial routes by land and water could be unsafe, *milites* and rich *censuales* might have been understandably reluctant to trust the disposal of their agricultural production to servants, and thus the link between the *possessor* and the merchant could be a close one.

The distinction drawn in this analysis between the great landholders—that is, the ecclesiastical and temporal lords—on the one hand, and the middling landholders on the other, is important from the tenth to the thirteenth centuries. In the agricultural sector this latter group throughout Northern Europe demonstrated its ability to adapt to the evolving money economy. Moreover, because in the first half of this two-hundred-and-fifty-year period social distinctions were still not clearly drawn below the level of the restricted group of nobles, among middling landholding families—especially their younger sons—economic opportunities outside agriculture were still relatively available. This does not mean that all the early patriciate and certainly not all the merchant element in that patriciate derived from families with landed wealth or revenues acquired from service to a great lord. Many of those who entered commerce doubtless came from the poorer elements of the countryside. But even these were not for the most part runaway serfs or foot-loose adventurers. In twelfth-century Amiens, for example, no family of distant origins left a lasting name in the city.⁷¹ For Picardy as a whole displacements of individuals

⁶⁹ In his "Origins of the Medieval Town Patriciate" (pp. 21–22) A. B. Hibbert makes an astute observation on Pirenne's analysis of the *monetarii* in the latter's early work, *Histoire de la constitution de la ville de Dinant au moyen-âge* (Ghent, 1889), 19–20.

⁷⁰ Quoted in Rolland, *Les origines*, 124 n.3.

⁷¹ Massiet du Biest, "Les origines de la population et du patriciat urbain à Amiens," *Revue du Nord*, 30 (1948): 113–32; Jean Lestocquoy, "Les villes et la population urbaine," *Cahiers*

and families in the period 1125–75 averaged between two to fifteen kilometers, with a few rare exceptions ranging from thirty to forty kilometers.⁷²

Most of the documentation, of course, for this earlier period comes from towns of pre-Norman foundation where the continuity of existence encouraged participation of indigenous elements in economic development. For towns like Douai, Ypres, and Lille, which grew up around a post-Norman castle and whose initial commercial growth appears to have come in the twelfth and especially in the thirteenth century,⁷³ it is very possible that numbers of settlers and the merchants among them came from a distance and that the origins of many of the evolving patrician families did not lie in the surrounding countryside. This is, however, pure hypothesis; the role of the agricultural establishment in the case of a “new” town like Brussels suggests the need for caution even here.⁷⁴ It is essential, nevertheless, when speaking generally, to stress the complexity of the early merchant group and the extent to which the countryside and landed wealth furnished capital for urban commerce and industry.⁷⁵ To deny a priori the possibility of commercial enterprise to landlords and officials of lay and ecclesiastical lords because of some predetermined mental orientation on their part is to distort the variegated nature of early urban society.

It is easier in the light of the complex composition of the early urban patriciate to understand the peaceful establishment of communal governments, which occurred all over the area bordered by the Loire and the Rhine in the eleventh and twelfth centuries. The old class-conflict thesis that saw in these communal movements the rise of a new class opposed to the establishment must be seriously modified. With the exception of relatively few cases like Cambrai and Laon, the establishment of the commune proceeded largely with the encouragement or the passive acceptance of town lords both lay and ecclesiastical. There was little about the communes that was revolutionary in intent.⁷⁶

de civilisation médiévale X–XII siècles, 1 (1958): 61–62; Edith Ennen, *Frühgeschichte der Europäischen Stadt* (Bonn, 1953), 189.

⁷² Fossier, *La Terre*, 1: 291.

⁷³ Even in the case of these new towns the entrepreneurial activity of a seigneur, the count of Flanders, must have been important. Jan Dhondt, “Développement urbain et initiative comtale en Flandre au XI^e siècle,” *Revue du Nord*, 30 (1948): 133–56.

⁷⁴ Guillaume Des Marez points out the role in the political and economic development of Brussels played by the Clutinc, a ministerial family holding lands in the place where the town grew up. *L'origine et le développement de Bruxelles: Le quartier Isabelle et Terarken* (Paris, 1927), 8.

⁷⁵ Petri comes to a similar conclusion, yet in my opinion tends to overstress the humble nature of the merchant group. “Die Anfänge des mittelalterlichen Städtewesens,” 267. The same criticism applies to van Houtte's modification of Pirenne's thesis. “Gesellschaftliche Schichten,” 263.

⁷⁶ Albert Vermeesch, in *Essai sur les origines et la signification de la commune dans le Nord de la France* (Heule, 1966), effectively criticizes the standard interpretation of the communal movement of the Middle Ages. He sees this interpretation, which views the commune as revolutionary and antifeudal, to be an imposition of a 1789 mentality upon the medieval past. For Vermeesch the commune was established basically as a peace-keeping institution. While in my opinion he goes to the other extreme in minimizing the violence that did occur in the

Admittedly, in the second half of the period under discussion the expansion of the class of nobles and the redefinition of the meaning of nobility involved in this phenomenon had the effect of making it more difficult for members of the middling landlord group to enter trade. In the course of the twelfth and thirteenth centuries the institution of knighthood, sanctified by the Church and glorified by poetry and elaborate ceremony, gradually came to attract into its membership men from the nobility. From being a profession, knighthood became the identifying attribute of the noble class. In part a product of the developing legalistic, ordering tendencies of the period, efforts at defining a code of conduct, dress rules, and other restrictions designed to set off the nobility from the other members of the society had two results. First, the number of families counted among the ranks of the nobility increased, and second, families in the middling landlord class were compelled to become part of the military aristocracy. Exclusion from the widened nobility would have pushed these families down into the now clearly defined ranks of the lower classes. Bloch was therefore accurate in pointing to an increasing class consciousness in these centuries.

I credit the ordering mentality of the period, however, as only partially responsible for the creation of the new exclusiveness among the nobility. In a sense the class consciousness of the military nobility was merely replacing another that focused on blood. The aggressiveness with which the new code of chivalry asserted itself seems in my opinion explicable on the grounds that, whereas the former nobility had been one of birth, with the new criteria families now could be "made" noble through knighthood. In theory the class was open toward the bottom, and the great problem was to set up restrictions and rules to shut off entrance. Paradoxically, essential to the understanding of the closing of the nobility is the recognition of the process of opening that preceded and helped to bring it about.

As might be expected, the assumption of the knights into the noble

course of setting up some of the communes, nevertheless his analysis of the movement, town by town, suggests that heretofore the extent of opposition to the movement by the urban lords has been vastly exaggerated. The continuity between the ruling elements in seigneurial and communal governments in the German towns has been emphasized by Philippe Dollinger, "Les villes allemandes au Moyen Age," *La ville: Recueils de la Société Jean Bodin*, 6 (1954): 456-62. For the ease of transition in Metz, see Jean Schneider, *La ville de Metz aux XIII^e et XIV^e siècles* (Nancy, 1950), 114-18. For instances of participation of *ministeriales* in the early commercial activities in the Rhine region, see Ganshof, *Étude sur les ministeriales*, 59-60. Knut Schulz brilliantly demonstrates the commanding role that the *ministeriales* played in the early economic development of medieval Trier and Worms. "Die Ministerialität als Problem der Stadtgeschichte," *Rheinische Vierteljahrsblätter*, 32 (1968): 184-219; and his *Ministerialität und Bürgertum in Trier: Untersuchungen zur rechtlichen und sozialen Gliederung der Trierer Bürgerschaft* (Bonn, 1968). For Nuremberg, see H. H. Hofmann, "Nobiles Norimbergenses," in *Untersuchungen zur Gesellschaftlichen Struktur*, 69. For the heterogeneous character of the patriciate of Andernach, Coblenz, and Bonn in the thirteenth century and their economic activities see Tadeusz Roslinowski, *Recherches sur la vie urbaine et en particulier sur le patriciat dans les villes de la moyenne Rhénanie septentrionale*, vol. 2 of *Studia z Dziejow Osadnictwa* (Warsaw, 1964): 112-18.

class obviously occurred at different times in different areas. In Namur, while the knights in the thirteenth century possessed social prestige, it was only at the end of the fourteenth century that they became fused with the old nobility.⁷⁷ In the bishopric of Liège the fusion came at the end of the thirteenth century,⁷⁸ and in Picardy the rise of the knights and the identification of nobility with knighthood was complete by the second quarter of the thirteenth century.⁷⁹ For regions further to the south the assimilation seems to have occurred earlier.⁸⁰ Nevertheless, it remains that the Parlement of Paris still found it necessary in the reign of St. Louis to prohibit lords from knightizing their serfs.⁸¹

Quite naturally, in defining the character and extent of this new class consciousness historians have too easily accepted the knight's behavior depicted by the poets and chroniclers of this period of assimilation as accurately describing the everyday life of the members of this new nobility. Unquestionably these authors represented the goals of the noble class in their creations, and the life and deeds of the men singled out for attention were idealized. To a degree the poets' picture of the knights has affected our conception of how the knight must have acted when confronted with the dangers of the evolving money economy. The very success of the middling landlords in an agricultural system increasingly dependent on markets should be telling proof against the reliability of much of this literature. While many nobles did ride the tournament circuit, seek out battles, allow their bailiffs to cheat them, or lost their manors through debt, there were others, probably the majority, who, while enjoying the privileges of their position, did little if any military service and spent their days in managing their lands. It is curious that Marc Bloch would choose the two constitutions of the Templar Order, the one drawn up in 1130 and the other in 1250, to show the growing class consciousness and exclusiveness of the nobles in these years.⁸² How really significant was the Templars' increasing sense of class with its implied antagonism to merchants and commerce, given the fact that the order was one of the most efficient agricultural entrepreneurs in Western Europe and the managers of one of the biggest banking operations?

One generally unrecognized source of vitality for the "closed" nobility of Northern Europe in the thirteenth century was curiously the great number of noble families that died out. The extinction rate for this class in

⁷⁷ Genicot, *L'économie rurale namuroise*, 2: 135.

⁷⁸ Poncet, *Oeuvres de Hemricourt*, 3: xcvi.

⁷⁹ Fossier, *La Terre*, 2: 661.

⁸⁰ Georges Duby, "Les origines de la chevalerie," in *Ordinamenti militari in occidente nell'alto medioevo* (Spoleto, 1968), 2: 739-61; see also the discussion of his paper, pp. 845-48. More extreme than any of the foregoing views is that of Léo Verriest, who maintains that at no time in the Middle Ages did knighthood make a man a noble. Rather he holds that apart from birth, nobility could be attained only by letters patent from the king. *Noblesse, chevalerie, lignages*, 53.

⁸¹ Perrin, *La seigneurie rurale*, 2: 218.

⁸² Bloch, *Feudal Society*, 320.

the Forez during this century was about thirty-one per cent.⁸³ In this province at the same time two or three generations of wealth were sometimes enough to raise a family into the ranks of the nobility. General recognition of noble status by the locality seems to have been the determining factor. Most of the new noble families were from the ranks of the successful peasants, while those of bourgeois origin had a harder time. To what degree did such high rates of extinction prevail in previous centuries, and to what extent can these conclusions be generalized for other areas of Europe? This was not the case in the Mâconnais before 1200, and evidence from Picardy indicates a relative stability of noble families there as well before the thirteenth century.⁸⁴ After 1200, however, not only the nobility of Picardy but also that of the Liège region clearly follows the pattern of the nobles of Forez.⁸⁵ Various explanations could be offered for the especially high extinction rate of the nobility in the thirteenth century, but one of the most likely is that many of the relatively new noble families had not yet had time to establish traditions designed to preserve the line, and the incidence of family mortality was consequently high among them. Whatever the explanation, the fact remains that in the thirteenth century the nobility in the countryside remained a powerful economic force in some degree owing to the constant incorporation into their number of the most successful members of the lower classes.

IN CONTEMPORARY SYNTHESSES of the economic and social history of medieval Europe I have been impressed by the resiliency of assumptions inherited from early liberal thought on the *ancien régime* and from Marx's exaggerations of some strands of that thinking. Modern economic and social historians of the Middle Ages are increasingly abandoning these assumptions in their detailed work, yet seem almost irresistibly drawn to the class-conflict thesis as a means for structuring general treatments of the economic and social life of the period. Admittedly there were tensions between town and country, merchant and landlord, especially in the thirteenth century, but thirty years of scholarship point to the conclusion that elements of discontinuity have been overemphasized at the expense of factors favoring continuity between ruling classes in urban and rural areas and the interdependency of the two sectors in the economic evolution. Not only throughout the period from 1000 to 1250 did the landlords profit from the price rise in agricultural products, but at least in the first half of that period the social structure of the society was such as to permit members of the middling group of landlords to participate in the initial stages of urban

⁸³ Édouard Perroy, "Social Mobility among the French Noblesse," *Past and Present*, 21 (1962): 31.

⁸⁴ Georges Duby, "Une enquête à poursuivre: la noblesse dans la France médiévale," *Revue historique*, 85 (1961): 22; Fossier, *La Terre*, 2: 663.

⁸⁵ Poncelet, *Oeuvres de Hemricourt*, 3: xcvi; Fossier, *La Terre*, 2: 663-64.

commerce and industry. Even in the thirteenth century, moreover, the landlords continued to prosper, and thus the hostility they felt toward the bourgeoisie appears to have been greatly exaggerated in the work of many modern historians. Consequently, rather than conceiving of the great economic progress of the period from 1000 to 1250 as largely achieved by the have-nots in spite and at the expense of the establishment, we would be more accurate in seeing it as a development in which the haves, in pursuit of even more, played the primary role.

The Egalitarian Myth and the American Social Reality: Wealth, Mobility, and Equality in the "Era of the Common Man"

EDWARD PESSEN

ALEXIS DE TOCQUEVILLE'S *Democracy in America*, a work whose insights into the American mind and soul have proven to be almost frighteningly prescient, has been justly acclaimed as the most penetrating single book yet written on American civilization. The book also contains the most influential as well as the most durable interpretation of Jacksonian America yet offered. For while older sectional and class analyses of the great political issues have been largely forsaken by scholars in our own time, Tocqueville's egalitarian version of Jacksonian society continues to command their wide support. The polemic and controversy that have marked the discussion of party battles in the age of Clay and Calhoun have been strangely missing from the consideration of the era's social developments. Jacksonians and anti-Jacksonians, now as earlier, subscribe to the Tocquevillean interpretation of antebellum American society.¹

This article is an expanded version of a paper given at the annual meeting of the American Historical Association, Dec. 29, 1970, in Boston, Mass. Grants from the Research Foundation of the State University of New York in 1969 and 1970 permitted me to do much of the research for the article. The staffs of many libraries, particularly those of the New-York Historical Society, the Long Island Historical Society, the New York City Municipal Archives and Records Center, the Massachusetts Historical Society, the Historical Society of Pennsylvania, the Department of Rare Books and Manuscripts of the Boston Public Library, and the Local History and Genealogical Division of the New York Public Library were splendidly cooperative. Useful ideas and information were contributed by John Daly, archival examiner of the City of Philadelphia, Dagoberto Molerio, teacher in the New York City public school system, Stuart Blumin of the Massachusetts Institute of Technology, David Spring of the Johns Hopkins University, Alice Hanson Jones of Washington University, and my colleague, Stanley Buder of Baruch College of the City University of New York. Samuel Richmond, professor of statistics and dean of the Graduate School of Business of Columbia University, made his formidable expertise available to me in the treatment of statistical problems. Students who assisted in my research and computations were Elizabeth Ann O'Brien, Andrew Pessen, Dinah Pessen, and Kenneth Jacobson. In selecting pictures, I am indebted to the valuable advice given by Wilson G. Duprey, curator of the Map and Print Room, New-York Historical Society, Nicholas B. Wainwright, director of the Historical Society of Pennsylvania, Miss Charlotte LaRue of the Museum of the City of New York, and Mrs. Beth Shub, graduate student in Brandeis University.

¹ Alexis de Tocqueville, *Democracy in America* (Paris, 1835, 1840). References in this article are to an edition edited by Phillips Bradley (New York, 1954). For useful recent discussions of the political historiography, see Alfred A. Cave, *Jacksonian Democracy and the Historians* (Gainesville, 1964); Charles G. Sellers, Jr., "Andrew Jackson versus the Historians," *Mississippi*

Tocqueville was not alone in depicting the American society of the second quarter of the nineteenth century in egalitarian terms. Many contemporaries, native Americans and European visitors alike, observed—or thought they observed—the same fluid social scene discerned by the brilliant young aristocrat.² Their joint perception of American society is Tocquevillean not in the sense that its central propositions were original with Tocqueville but rather because no one else formulated the egalitarian thesis so comprehensively, so lucidly, so logically as he. Tocqueville's social portrait is a model of internal consistency.

According to the egalitarian thesis the United States was a society dominated by the great mass of the people, who composed the middling orders. Unfortunate minorities aside, few men here were either very poor or very rich. For that matter the rich here were rich only by American standards, their wealth not comparing in magnitude to the great fortunes accumulated by wealthy European families. What rich men there were in America were typically self-made, born to poor or humble families. Nor did they long hold on to their wealth. Flux ruled this dynamic society; riches and poverty were ephemeral states in this kaleidoscopic milieu. The limited extent and the precariousness of wealth helped explain the dwindling influence of its possessors. At a time when the most liberal of European states was grudgingly permitting some wealthy bourgeois to share the suffrage with great landholders, America was brushing aside all important restrictions on voting. Deference gave way to the strident rule of the masses, as the beleaguered rich turned their backs on a politics permeated by vulgarity, opportunism, and other loveless expressions of popular power. Social and economic democracy followed on the heels of, as they were in part caused by, political democracy. In a society that exalted work over status, class barriers loosened and diminished in significance. And a near if not a perfect equality of condition resulted from the unparalleled equality of opportunity that in Tocqueville's time complemented the abundant natural resources, the technological advances, and the human energy that had been present in America since its settlement.³

Valley Historical Review, 44 (1958): 615–34; and Edward Pessen, *Jacksonian America: Society, Personality, and Politics* (Homewood, 1969), 352–93. Among the modern works that subscribe to one or more of the central tenets of the egalitarian theory are David M. Potter, *People of Plenty* (Chicago, 1958); Carl N. Degler, *Out of Our Past* (rev. ed.; New York, 1970), 144–45; Marvin Meyers, *The Jacksonian Persuasion* (Stanford, 1957); Lee Benson, *The Concept of Jacksonian Democracy: New York As a Test Case* (Princeton, 1961); Marcus Cunliffe, *The Nation Takes Shape, 1789–1837* (Chicago, 1965); John William Ward, "The Age of the Common Man," in John Higham, ed., *Reconstruction in American History* (New York, 1962), 82–97; David Riesman, Nathan Glazer, and Reuel Denney, *The Lonely Crowd* (New York, 1953); Rowland Berthoff, "The American Social Order: A Conservative Hypothesis," *AHR*, 65 (1959–60): 499; and Stuart Bruchey, *The Roots of American Economic Growth, 1607–1861* (New York, 1968).

² For a discussion of the individuals who helped shape the egalitarian viewpoint, see Pessen, *Jacksonian America*, 39–46.

³ Tocqueville, *Democracy in America*, 1: 53–54; 2: 105, 138, 164, 199, 234, 237, 239, 250, 251, 258, 263, *passim*.

Tocqueville's comprehensive evaluation of American civilization, stressing as it did the ambivalent values and beliefs of the people, was of course more complex than the preceding passage suggests. The purpose of this essay, however, is not to assess the subtle social philosophy of Alexis de Tocqueville but rather to consider those elements in his appraisal of America that have been widely subscribed to and that form what I call the egalitarian thesis.

The egalitarian thesis has not gone unchallenged. A few contemporary men of affairs joined spiritual and secular perfectionists and labor radicals in dissenting from the consensus as a whole or from particular parts of it. A number of recent publications have also questioned one or another aspect of the traditional interpretation.⁴ It is no disparagement to note that the modern criticisms have for the most part focused on other themes: they have either touched on only one facet of the egalitarian thesis or examined only one community; and they have been impressionistic in their methodology.⁵ That is, their data are random and inconclusive.

I have gathered much evidence—or what nowadays is called “quantitative data”—from major cities of the Northeast in order to subject the thesis of antebellum egalitarianism to the kind of detailed check it has hitherto been largely spared. What follows is a report on how well several fundamental axioms of the egalitarian theory stand up to the test of empirical verification. Since it is better to be intelligible than modish, clear prose and simple mathematical terms have been used wherever possible in preference to esoteric charts or terminology.⁶ Serious historians, even those ancients who

⁴ For a discussion of some of the era's dissenters, see Arthur E. Bestor, Jr., *Backwoods Utopias* (Philadelphia, 1950), and Edward Pessen, *Most Uncommon Jacksonians* (Albany, 1967). For recent publications, see Richard C. Wade, *The Urban Frontier: The Rise of Western Cities, 1790-1830* (Cambridge, 1959), 203-30; D. Clayton James, *Antebellum Natchez* (Baton Rouge, 1968); Kenneth W. Wheeler, *To Wear a City's Crown: The Beginnings of Urban Growth in Texas, 1836-1865* (Cambridge, Mass., 1968); Robert A. Dahl, *Who Governs? Democracy and Power in an American City* (New Haven, 1961); Douglas T. Miller, *Jacksonian Aristocracy: Class and Democracy in New York, 1830-1860* (New York, 1967); Miller, “Immigration and Social Stratification in Pre-Civil War New York,” *New York History*, 49 (1968): 157-68; Pessen, *Jacksonian America*; Sidney H. Aronson, *Status and Kinship in the Higher Civil Service* (Cambridge, Mass., 1964); and Gary B. Nash, “The Philadelphia Bench and Bar, 1800-1861,” *Comparative Studies in Society and History*, 7 (1965): 203-20.

⁵ Exceptions to the impressionistic approach are a number of recent quantitative studies of particular communities, some of them yet unpublished. Particularly valuable are Stuart Mack Blumin, “Mobility in a Nineteenth-Century American City: Philadelphia, 1820-1860” (Ph.D. dissertation, University of Pennsylvania, 1968), a condensed summary of which appears under the title “Mobility and Change in Ante-Bellum Philadelphia,” in Stephan Thernstrom and Richard Sennett, eds., *Nineteenth-Century Cities: Essays in the New Urban History* (New Haven, 1969), 165-208; Alexandra McCoy, “The Political Affiliations of American Elites: Wayne County, Michigan, 1844-1860, as a Test Case” (Ph.D. dissertation, Wayne State University, 1965); and two papers read at the annual meeting of the Organization of American Historians, April 19, 1971, in New Orleans: Robert Doherty, “Property Distribution in Jacksonian America,” on New England; and Michael B. Katz, “Patterns of Inequality, Wealth and Power in a Nineteenth-Century City,” on Hamilton, Ontario.

⁶ In dealing with the distribution of wealth, for example, it has appeared to me as sensible and more economical to make simple statements of the proportions of wealth owned by different levels of wealth holders than to use the Lorenz curve as the means of indicating graphically the difference between a perfect equality of distribution and the actual inequality

had the quaint notion that their purpose was to tell an interesting story well, did much research in the evidence after all. It would be dismaying to think that the amassing of data obliges the scholar to foresake plain English for a mode of communication more characteristic of the computer that may have abetted his research than of the historian who must communicate his findings and his audience who must read them.

"IN A DEMOCRATIC SOCIETY like that of the United States," wrote Tocqueville, "fortunes are scanty," since "the equality of conditions [that] gives some resources to all the members of the community . . . also prevent any of them from having resources of great extent."⁷ Tocqueville's reasoning was flawless. As always the great question is whether his logic is borne out by the facts.

It is of course impossible to define objectively "resources of great extent" or riches. The terms may be said to contain both a relative and an absolute component. Riches or great wealth will be owned by relatively few, giving each of their possessors a portion of the community's goods that equals or surpasses in value the total wealth owned by hundreds or even thousands of poorer men. And riches make possible lives marked by material comfort, costly even sumptuous possessions, servants or retainers to perform menial tasks, much leisure time and attractive and expensive means of spending or using it. Since a number of contemporary sources indicated the quantity of wealth required to live the life of the rich, the question to be answered concerns the numbers who attained such wealth.

Digging out evidence of this sort is, however, much harder than devising plans to do so. That few scholarly tasks are more difficult than determining the precise wealth owned by individuals at a given point in their lifetimes prior to death is made abundantly clear by the admitted inexactness of the estimates of wealth made by the few outstanding economic historians who have tried to deal with the problem. Authorities in this esoteric field offer not precise attributions of wealth but informed estimates.⁸

that inevitably obtains. It is perhaps more revealing to say that the poorest ninety per cent of Philadelphians owned only ten per cent of the wealth in 1860 than to note that the Schutz coefficient of inequality for the same data is .79 (with 0 representing perfect equality and .999 the greatest possible degree of inequality). Similarly, I fail to see the need to refer to the Gini coefficient of concentration, which would be zero in the case of perfect equality of distribution and approaches closer to 1.0 as the area between the diagonal line, representing perfect equality of distribution (in a graph in which the vertical pole measures wealth and the horizontal, population) and the Lorenz curve (connecting the points that indicate the proportions of wealth owned by varying percentages of the population—"deciles," where the latter are divided into tenths) enlarges, in an instance in which the richest tenth owning better than forty per cent of the wealth and the poorest half less than fifteen per cent, produces a Gini coefficient of .54.

⁷ *Democracy in America*, 2: 250, 258. Writing more recently, Marcus Cunliffe advises that in America "sizable fortunes were made, but not astronomical fortunes and not static fortunes." *The Nation Takes Shape*, 169.

⁸ See Ralph Hidy, *The House of Baring in American Trade and Finance: English Merchant*

Nor have many attempts been made by scholars to date to fix the wealth of the great accumulators.⁹ In effect the supply of authoritative information is slim, while what is authoritative is unavoidably inexact. If precise scholarly appraisals are lacking, there exists interesting contemporary published evidence that in effect takes up the question: were there very few or no Americans who had substantial fortunes during the era?

Not according to the estimates made by Moses Yale Beach, erstwhile publisher of the *New York Sun* and of lists of New York City's wealthiest one thousand during the 1840s and 1850s.¹⁰ In an 1845 edition of his list John Jacob Astor's wealth was placed at \$25,000,000 while twenty other eminent men were adjudged millionaires. Another one thousand New Yorkers were estimated to be worth varying sums ranging from \$100,000 to \$800,000 for each. The problem with Beach's rating, however, is that in addition to its careless errors and the mystery of its sources—faults noted but accepted by the many scholars who used it because they believed they had no alternative—it was riddled with inconsistencies, was put together shoddily if not

Bankers at Work, 1763-1861 (Cambridge, 1949), 40, 46; Kenneth W. Porter, *John Jacob Astor, Business Man* (New York, 1966), 2: 939n.; and Philip L. White, *The Beekmans of New York in Politics and Commerce, 1617-1877* (New York, 1956), 214. Wills, while useful, disclose only what a man was worth at his death—if they do that. In many cases wills affixed no monetary value to the items they listed. See, for example, Leo Hershkowitz, comp., *Wills of Early New York Jews, 1704-1799* (New York, 1967), or the will of William Hamersley, in Lawrence Roth, ed., *Colonial Families of America* (New York, 1946), 25: 16. While probate inventories are invaluable, they, too, are imperfect, typically omitting real property as well as transfers of property before death, depending on the ability of particular valuers, and not existing in sufficient numbers to permit a comprehensive comparison of the relative wealth owned by diverse individuals and wealth-holding groups in a given community. See, for example, "Inventory of the Personal Estate of Peter G. Stuyvesant, December 30, 1847," Stuyvesant Papers, New-York Historical Society. For critiques of the weaknesses of the inventories by scholars who have made effective use of the inventories' strengths, see Jackson Turner Main, *The Social Structure of Revolutionary America* (Princeton, 1965), 288-91; Richard Grassby, "The Personal Wealth of the Business Community in Seventeenth-Century England," *Economic History Review*, 2d ser., 23 (1970): 220; and Alice Hanson Jones, "Wealth Estimates for the American Middle Colonies, 1774," *Economic Development and Cultural Change*, 18 (1970): x, 1-172. Mrs. Jones makes brilliant use of slightly more than two hundred inventories in effect to infer the proportions of wealth held by all levels of wealth holders, a process that of course could not be used to determine the wealth held by particular living individuals.

⁹ Peter N. Stearns advises me that the extant European literature is sparse. The contemporary *Zeitgeist* may be a contributing factor, since for England, he has written me, "for the most part social historians have stressed the poor rather than the rich in their studies." Whatever the reasons, the relevant bibliography is a small one. The situation evidently has not changed very much from what it was almost a half century ago, when Sorokin wrote that "wealthy men as a specific social group have been studied very little up to this time." Pitirim A. Sorokin, "American Millionaires and Multimillionaires: A Comparative Statistical Study," *Journal of Social Forces*, 3 (1925): 627. Sorokin's method for determining "millionaires," particularly for the nineteenth century, relied heavily on surmise. Robert E. Gallman has recently estimated that there were sixty "millionaire families in the United States in 1840," but unfortunately his estimate is based on undocumented contemporary listings of "wealthy citizens." Gallman, "Trends in the Nineteenth Century: Some Speculations," in Leo Soltow, ed., *Six Papers on the Size Distribution of Wealth and Income* (New York, 1969), 15. See below for a discussion of the various Wealthy Citizens listings.

¹⁰ In New York, beginning in 1842, Beach and his son published thirteen editions of a pamphlet that typically contained the names, the estimated wealth, and biographical vignettes of the purportedly one thousand wealthiest citizens of New York City. The title most often used for these listings was *Wealth and Biography of the Wealthy Citizens of New York City*.

irresponsibly, relied on much guesswork, evidently had little or no standing with informed contemporaries, and omitted hundreds of persons who were worth at least the minimum sum required by Beach for inclusion in his pamphlet.¹¹ Lists similar to Beach's, published for Philadelphia, Brooklyn, Boston, and a number of New England towns, also claimed large fortunes for various luminaries, but the cryptic treatment of the sources of these estimates does not inspire confidence in their accuracy.¹² Probate inventories, while invaluable for particular individuals and general trends, were certain to be incomplete as a clue to the wealth owned by a large group of persons. In the absence of other reliable data the manuscript tax assessment records of the nation's wealthiest cities were consulted, therefore, in order to determine who were the rich and how great were their fortunes.

Urban residents were taxed not on incomes but on the total wealth, real and personal, they owned within the city. During the era Boston printed annual records of all persons who paid taxes on twenty-five dollars or more. New York City, unfortunately, did not. Lists of New York City's taxpayers for 1828 and 1845 were thus created out of the unindexed assessment data: over 100,000 separate items, which in their raw form simply listed, street by street, location by location, the assessed value of each property or estate owned in the city, and alongside it the name of its owner, agent, trustee, administrator, or executor. A John Jacob Astor, who owned hundreds of separate properties, appeared hundreds of times in the original records. Similar lists were drawn up for the then separate—and wealthy—city of Brooklyn.¹³ (Philadelphia's tax records did not permit such treatment, since assessors there did not distinguish between owners and users of real property.)

Such lists, if not requiring "a lifetime of research," were indeed time con-

¹¹ See Edward Pessen, "Moses Beach Revisited: A Critical Examination of His Wealthy Citizens Pamphlets," *Journal of American History*, 58 (1971): 415-26.

¹² Among the listings were "A Member of the Philadelphia Bar," *Wealth and Biography of the Wealthy Citizens of Philadelphia* (Philadelphia, 1845); "A Merchant of Philadelphia," *Memoirs and Auto-Biography of Some of the Wealthy Citizens of Philadelphia* (Philadelphia, 1846); John Lomas and Alfred S. Peace, *The Wealthy Men and Women of Brooklyn and Williamsburgh* (Brooklyn, 1847); William Armstrong, *The Aristocracy of New York: Who They Are and What They Were* (New York, 1848); Thomas L. Wilson, *Aristocracy of Boston* (Boston, 1848), a unique listing in that it offered no estimates of wealth; A Forbes and J. W. Green, *The Rich Men of Massachusetts* (Boston, 1852); and "Our First Men": *A Calendar of Wealth, Fashion and Gentility* (Boston, 1846). The authorship of the latter pamphlet has been attributed to the historian Richard Hildreth. See Donald E. Emerson, *Richard Hildreth* (Baltimore, 1946), 126, 169. A search by Miss Dinah Pessen in the Harvard Library failed to uncover in Hildreth's "Literary Memoranda" the evidence that is the basis of Emerson's contention that Hildreth wrote the *Calendar*. "Our First Men" was published by the printers David H. Ela and Abner Forbes. Ela was a lawyer who had served in the Treasury Department and written on financial subjects; see the *New England Historical and Genealogical Register*, 79 (1926): 89-90. In a letter written December 6, 1892, Frank H. Forbes, son of Abner Forbes, claimed that he and his father had written the pamphlet (which he does not name by title); the letter is located in the Department of Rare Books and Manuscripts of the Boston Public Library.

¹³ The New York City assessment records for the period are located in the New York City Municipal Archives and Records Center. The Brooklyn assessments for 1810 and 1841 are at the Long Island Historical Society.

suming to prepare. Their redeeming value is that they make possible not only the creation of reliable lists of comparative wealth,¹⁴ but answers to a number of important questions raised by the egalitarian thesis.

The assessments have a number of weaknesses, the chief one being their undervaluation of the estates, particularly the personal estates of wealthy men.¹⁵ Tax officials bemoaned the practice of great merchants, men known to be owners of vast real-estate holdings and substantial shareholders in banks and insurance companies, coolly to swear that they possessed no personal wealth whatever. Who could say them nay in view of the incorporeality and the impossibility of tracking down this form of wealth? Boston's property owners may have been unusual in disclosing to assessors much personal wealth, yet officials even in that highly moral city were rightly convinced that property there was assessed substantially below its market value.¹⁶ The consoling feature of the sums disclosed by the tax records is that they are solid bedrock. Charles Hoyt in 1841 was worth at least the \$242,226 Brooklyn's assessors of his real property said he was; Hezekiah B. Pierrepont was worth at least the \$629,000 that his many lots were assessed for—although if the poor chap could be believed he had not a penny in liquid assets.

Even when the assessed valuations are taken at face value, New York City at the time of Tocqueville's visit had about one hundred persons worth \$100,000 or more, while Boston had seventy-five worth at least that sum. A decade later, shortly after the second volume of *Democracy in America* appeared, New York's tax data disclosed that John Jacob Astor and Peter G. Stuyvesant were millionaires, while three hundred other persons were worth \$100,000 or more. Boston by then had 150 individuals worth the latter sum, in addition to Peter Chardon Brooks, the millionaire.¹⁷ Brooklyn, by 1841 the nation's seventh city, had twenty-six individuals assessed for \$100,000 or more, if none at one million. While Philadelphia's assessments, in not distinguishing between owners and users of real property, do not indicate the total assessed wealth owned by citizens within the city, there is evidence that the Wealthy Citizens listing for that city, like the similar lists for Boston and Brooklyn, was reliable in important respects.¹⁸ Its

¹⁴ See Edward Pessen, "The Wealthiest New Yorkers of the Jacksonian Era: A New List," *New-York Historical Society Quarterly*, 54 (1970): 145-72.

¹⁵ For a discussion of the strengths and weaknesses of tax data as a clue to the wealth of individuals, see Pessen, "The Wealthiest New Yorkers of the Jacksonian Era," 148-52, and note 23 below.

¹⁶ Before 1842 assessments were openly made at one-half the estimated value of property. See Lemuel Shattuck, *Report to the Committee of the City Council Appointed to Obtain the Census of Boston for the year 1845* (Boston, 1846), app., 59; William Minot, Jr., *Taxation in Massachusetts* (Boston, 1877), 7; and *Report of the Committee to Consider . . . Measures . . . that will improve the method of assessing, abating and collecting the Taxes of the City*, City Doc. No. 9 (Boston, 1848).

¹⁷ See *List of Persons, Copartnerships, and Corporations, Taxed in the City of Boston for the Year[s] 1833 [and 1843]* (Boston, 1834, 1844).

¹⁸ A detailed examination of Lomas and Peace's Brooklyn list shows that most of its estimates were close to the assessed valuations of wealth. As for Boston's *Calendar of Wealth*,

publisher claimed that Philadelphia had eleven millionaires and another 350 persons each worth \$100,000 or more. In any case, the death in 1831 of Stephen Girard, that city's great banker, revealed that he was worth more than \$6,000,000, or almost precisely what he was listed for.¹⁹

The \$100,000 figure that many hundreds of Northeasterners were assessed at may not appear to be an impressive sum. Yet even if one makes the most unrealistic assumption that the assessment figures accurately recorded the extent of an individual's wealth, the sums in question were hardly paltry. According to John Jacob Astor's grandson—and he was in a good position to know—a member of the "exclusives" could in 1850 have devoted himself entirely to the good life, including leisurely travel in Europe, on ten thousand a year, in "dollars not pounds," as he hastened to add.²⁰

The dollar of the 1830s was capable of wondrous things. William E. Dodge was able to rent a two-story house on Bleeker Street in New York City for an annual rental of \$300, while one or two hundred dollars more

Fashion and Gentility, approximately 85 per cent of the close to 400 individuals it listed were among the city's 420 most heavily assessed persons. (By way of contrast, fewer than 50 per cent of Moses Beach's "top 1,000" were among the 1,000 New Yorkers assessed for the most wealth.) Guided by the ingenious methodology of Blumin's "Social Mobility in a Nineteenth-Century American City," which identified a small number of occupations and residential districts with great wealth, I have checked the occupations and residences of the 1,128 individuals listed in the *Memoirs and Auto-Biography of . . . the Wealthy Citizens of Philadelphia*, for occupation and residence. Not only was the pamphlet accurate in recording occupations—that is, it was borne out by directories and other evidence—the occupations were overwhelmingly the high status occupations associated with great wealth. Seventy-seven per cent were classified as merchants; 11 per cent as lawyers and doctors; 6 per cent as manufacturers; and only one per cent as "mechanics"—some of the latter of whom might well have been entrepreneurs. Compare Jackson Turner Main's finding that of Philadelphia's richest 100 in 1765, "more than half . . . were merchants. Nearly one out of five were professional men, mostly lawyers and doctors. . . . Less than one-tenth were artisans or manufacturers." *The Social Structure of Revolutionary America*, 192. Of the 800 wealthy Philadelphians whose residences were located, a majority lived in the wealthy eastern wards whose per capita assessed wealth was more than three times as great as the wealth of most of the other wards in the city. That a house was not located between the Delaware and Seventh Street, running east to west, or between Mulberry and Walnut Streets, north to south, was hardly a sign of the poverty of its owner, however. By the 1840s many of Philadelphia's wealthiest and most notable persons lived in "Girard Row," the row of houses on the north side of Chestnut Street, between Eleventh and Twelfth Streets. According to Willis P. Hazard, who published a revised edition of John F. Watson's *Annals of Philadelphia in the Olden Time* (Philadelphia, 1927), 3: 247, although the eastern wards were still substantially wealthier than the western in 1835, a sharp narrowing of the gap between them had begun by 1829 and continued thereafter. Many a "mansion" stood in the "poorer" wards of Philadelphia, the wealth of the mansion's occupant not sufficient to change significantly the average wealth of all of the district's inhabitants. (This pattern existed in Brooklyn as well. Although Brooklyn Heights was clearly the center of fashion and wealth, many wealthy persons continued to inhabit homes put up by their families generations earlier, in what had now become poorer neighborhoods. See Edward Pessen, "A Social and Economic Portrait of Jacksonian Brooklyn," scheduled for publication in the October 1971 issue of the *New-York Historical Society Quarterly*.) Another indirect sign of the Philadelphia list's worth is the fact that it contained about 85 per cent of the persons who belonged to the exclusive Philadelphia Club in the mid-1840s.

¹⁹ Stephen Simpson, *Biography of Stephen Girard* (Philadelphia, 1832), 213; Harry Emerson Wildes, *Lonely Midas: The Story of Stephen Girard* (New York, 1943), 247.

²⁰ Charles Astor Bristed, *The Upper Ten Thousand: Sketches of American Society* (New York, 1852), 18. According to Bristed, an upper 10,000 was a great exaggeration, "for the people so designated are hardly as many hundreds" (p. 271).

could pay for an elegant place on "aristocratic Park Place among the Motts, Hones, Costers, Haggertys, Austins, Beekmans, and Hosacks," the *crème de la crème* of New York City society.²¹ Room and board at the new Astor House cost \$1.50 in 1836, that sum paying for four meals consisting of "all the delicacies of the season . . . served in a most ample manner." Philip Hone, who was sufficiently demanding a gourmet to have found the famed Delmonico's Restaurant wanting, though the fare at the Astor House capital; he had never seen "a table better set out, better provided, or a dinner better cooked." A wealthy Philadelphian of mid-century held that fifty dollars "constituted the millionairism of money aristocracy of those days," since this sum enabled a man to keep a carriage. According to Sidney George Fisher of Philadelphia, his annual income of less than \$3,000 gave him "a comfortable house—servants, a good table—wine—a horse—books—'country quarters,'—a plentiful wardrobe—the ability to exercise hospitality," while an additional one thousand would have enabled him to live like a truly rich man. In view of the prices of other representative goods and services, one understands better why as late as 1852 an informant could advise Carl Schurz that in New York City \$150,000 was considered a fortune.²² There is good reason to believe that men assessed at \$100,000 were typically worth many times that sum.

In the absence of income taxes, as well as the presence of a local tax that characteristically took less than one per cent of what a man claimed to be worth, assessed wealth of \$100,000 made him a functional millionaire several times over, in terms of modern costs and prices. His real estate was undervalued conservatively by half. Personal estate, regarded by tax authorities and insiders as typically equal in value to real, was almost totally masked. Possessions and investments outside the city were treated by assessors as non-existent. Not one penny was yielded up to a federal tax bureau that in our own era appropriates a substantial portion of a rich man's wealth. In view of the fact, finally, that the dollar of 1840 appeared to be worth roughly between five and six and one-half dollars of 1970, wealth that 130 years

²¹ Abram C. Dayton, *Last Days of Knickerbocker Life in New York* (New York, 1871), 97; William E. Dodge, *Old New York: A Lecture* (New York, 1880), 17. To compare the cost of living for a Western merchant, see Daniel Aaron, "Cincinnati, 1818-1838: A Study of Attitudes in the Urban West" (Ph.D. dissertation, Harvard University, 1942), 67.

²² The latter quotation is from Robert G. Albion, *The Rise of New York Port, 1815-1850* (New York, 1939), 258. The "Philadelphian" is cited in Elizabeth M. Geffen, "Joseph Sill and His Diary," *Pennsylvania Magazine of History and Biography*, 94 (1970): 303. Fisher's comment is from the diary of Sidney George Fisher, Jan. 17, 1842, Historical Society of Pennsylvania. Other sources are Benjamin Silliman, "Personal Reminiscences of 60 Years at the New York Bar," in David McAdam *et al.*, eds., *History of the Bench and Bar of New York* (New York, 1897), 1: 232; *History of the Chemical Bank, 1823-1913* (New York, 1913), 118-19; Freeman Hunt, *Lives of Eminent Merchants* (New York, 1857), 1: 475-76; Asa Greene, *A Glance at New York* (New York, 1837), 18; I. N. Phelps-Stokes, *The Iconography of Manhattan Island* (New York, 1916, 1918, 1922, 1926, 1928), 3: 528; and the MS diary of Philip Hone, 13: 249. The twenty-eight folio volumes are at the New-York Historical Society. The massive unpublished segments in particular, which constitute about ninety per cent of Hone's account, are a treasure trove on life among New York City's social and economic elite.

ago was assessed at \$100,000 is the equivalent of about forty times that gross amount in our own day.²³

In 1845, when John Jacob Astor's assessed wealth came to \$3,074,705, there is good reason to think that he was worth at least five times that sum.²⁴ From a Philip Hone notation that "Mr. Astor once remarked that riches did not bring happiness, a man may be as happy with half a million as if he was rich,"²⁵ one can decide for himself how many times "half a million" the author of that statement was worth. The great merchant banking house of

²³ It is impossible to make an exact comparison of the value of the dollar in 1840 and 1970. It was possible, however, to work out a rough comparison by using reliable, mainly federal data on changing wholesale and consumer price indexes that trace the changing value of the dollar. My estimate that the 1840 dollar was worth about \$6.50 in 1970 is based on Arthur Harrison Cole, *Wholesale Commodity Prices in the United States, 1700-1861* (Cambridge, Mass., 1938); Horace G. Wadlin, Chief of U.S. Bureau of Statistics, *Comparative Wages and Prices, 1860-1897* (Boston, 1898); U.S. Department of Labor, Bureau of Labor Statistics, *Index Numbers of Wholesale Prices on Pre-War Base, 1890 to 1927* (Washington, 1928); Robert A. Sayre, National Industrial Conference Board, *Consumers' Prices 1914-1948* (New York, 1948); U.S. Senate, Joint Economic Committee Report, with U.S. Department of Labor, Bureau of Labor Statistics, *Frequency of Changes in Wholesale Prices: A Study of Price Flexibility* (Washington, 1959); and information on 1956-70 furnished me by the U.S. Department of Labor, Bureau of Labor Statistics, Regional Office II, for New York, New Jersey, Puerto Rico, and the Virgin Islands. Using different data, Alice Hanson Jones has worked out a devaluation from 1774 to 1967, in which a later dollar is worth one-sixth the earlier. "Wealth Estimates for the American Middle Colonies," 127-29. She advises me, I am glad to say, that in the judgment of the authoritative Jack E. Triplett, neither estimate is right or wrong; different data yield different conclusions; perfection is out of the question in price estimates. See Jack E. Triplett, "Quality Bias in Price Indexes and New Methods of Quality Measurement," in Zvi Griliches, ed., *Price Indexes and Quality Changes*, scheduled for publication in 1971. It is also consoling that in Mrs. Jones' judgment the difference between her skilled estimate and my rough effort is not significant. The changing price index undoubtedly underestimates the devaluation that has occurred in the "rich man's dollar." Mrs. Jones, who was formerly with the Department of Labor, informs me that the CPI "is the price of a 'market basket' of goods and services commonly purchased by wage earners and clerical workers." (My italics.) My formula for estimating the contemporary gross-dollar value of the rich man's assessed wealth of 130 years ago treats personal estate as equal in value to real, and out-of-city real holdings as worth one-half of city properties. See John C. Schwab, *History of the New York Property Tax: An Introduction to the History of State and Local Finances in New York* (Baltimore, 1890), 87; *Report of the Special Committee of the Common Council of the City of Brooklyn, on Finances, Indebtedness, etc. of the City* (Brooklyn, 1838). In Hone's notes the prices realized by a number of wealthy New Yorkers on the sale only of portions of their city properties were more than twice the amount of their total wealth as reported by assessors. The charge that assessments measured only a small fraction of the wealth of the rich appears in *Minutes of the Common Council of the City of New York, 1784-1831* (New York, 1917), 8: 437; New York State, *Report of the Tax Commissioners of New York* (New York, 1871), 30; "Report of the Special Committee of the Board of Supervisors Appointed to examine the Assessment Rolls, and ascertain whether valuations in each Ward bear a just relation to the aggregate valuations in all the Wards," New York City, Sept. 4, 1829, in City Clerk Filed Papers, Location 3012, New York City Municipal Archives and Records Center; G. N. Bleecker, Comptroller, "Communication from the Comptroller on Subject of the Defective Manner of Assessing Personal Property in This City," New York, Jan. 24, 1820, City Clerk Filed Papers, Location 3216; and in other sources discussed in Pessen, "The Wealthiest New Yorkers of the Jacksonian Era," 151 n.9.

²⁴ The leading student of Astor's finances, Kenneth W. Porter, is skeptical of Astor's claim that his real estate in 1846 was worth only \$5,184,340. Porter notes that seven years earlier the great merchant conceded that his real property was worth \$5,445,525; subsequently he amassed more property, while the value of his older properties had appreciated. *John Jacob Astor*, 2: 939, 951-52.

²⁵ MS diary of Philip Hone, 22: 356.

Brown Brothers, led by Alexander Brown of Baltimore and his sons, one of whom, James, was resident in New York City, and another, John A., in Philadelphia, was regarded as one of the greatest international banks; by the mid-1830s its various branches were worth at least \$6,000,000.²⁶ Nor can there be any question that such families as the Stuyvesants, Lorillards, Whitneys, Rhinelanders, Kings, Brookses, Appletons, Greenes, Lawrences, Pierreponts, and Ridgways were then each worth \$1,000,000 or more.

Even before the end of the eighteenth century Thomas Willing, the Philadelphia financier, and William Bingham had each accumulated millions.²⁷ By the 1830s hundreds of families in the nation's Northeastern cities had amassed great fortunes based on commerce, insurance, finance, ship-building, manufactures, landholding, real-estate speculation, and the professions. The resources and the style of living enjoyed by the Eckfords, Masons, Hendrickses, Beekmans, Lenoxes, Joneses, Costers, and Gouverneurs in New York City, the Bownes, Bergens, Howlands, Lefferts, Cortelyous, Trotters, and Hickses in Brooklyn, the Brimmers, Codmans, Cushings, Parkmans, Phillipses, Quincys, Greenes, Derbys, Otises, Searses, Shaws, Welleses, and Williamses in Boston, and the Becks, Copes, Cadwaladers, McKeans, Walns, Shippens, Biddles, Whartons, Wetherills, and Willings in Philadelphia, not to mention the great families of the antebellum South, were very far from scanty and would have been regarded as substantial wealth anywhere in the world.

The wealth of the American rich, unlike that of the contemporary English aristocracy, derived largely from commerce and when from land, often took the form of real properties recently accumulated in speculations rather than estates held in the family for centuries as was the case with the Bedfords, Northumberlands, or Devonshires overseas.²⁸ The American rich were a working class, however, only in a technical rather than an actual sense. The preference shown by many of them for attending to business rather than to pleasurable uses of leisure was a matter of taste rather than

²⁶ The Philadelphia Wealthy Citizens list modestly estimated Brown's wealth at \$500,000. See the letter of an officer of the firm to the governor of the Bank of England, cited in John Crosby Brown, *A Hundred Years of Merchant Banking: A History of Brown Brothers and Company* (New York, 1909), 83; see also David S. Landes, *Bankers and Pashas: International Finance and Economic Imperialism in Egypt* (London, 1958), ch. 1, and Benson J. Lossing, *History of New York City* (New York, 1884), 2: 583.

²⁷ Burton Alva Konkle, *Thomas Willing and the First American Financial System* (New York, 1937), 120-21; Robert C. Alberts, *The Golden Voyage: The Life and Times of William Bingham, 1752-1804* (Boston, 1969), ix, 429; Henry Simpson, *The Lives of Eminent Philadelphians* (Philadelphia, 1859), 87; Margaret L. Brown, "Mr. and Mrs. William Bingham of Philadelphia," *Pennsylvania Magazine of History and Biography*, 61 (1937): 286-324; Brown, "William Bingham, Eighteenth Century Magnate," *Ibid.*, 387-434; William Otis Sawtelle, "William Bingham of Philadelphia and His Maine Lands," *Publications of the Genealogical Society of Pennsylvania*, 9 (1926): 207-26.

²⁸ My view of English landed wealth is derived mainly from F. M. L. Thompson, *English Landed Society in the Nineteenth Century* (London, 1963); David Spring, *The English Landed Estate in the Nineteenth Century: Its Administration* (Baltimore, 1963); and from conversations in which Mr. Spring was kind enough to give me the benefit of his unsurpassed knowledge of the Bedford and other English landed families.

necessity and can be compared to the preoccupation shown by some peers with the details of managing their estates. By almost any criterion opulent Americans lived lives comparable to those enjoyed by their English and Continental counterparts and were evidently able to do so with significantly smaller expenditures of money. (The ten thousand dollars per annum said by William B. Astor's nephew to be required for lavish living was evidently from one-third to one-quarter the amount needed to achieve a similar standard abroad.)

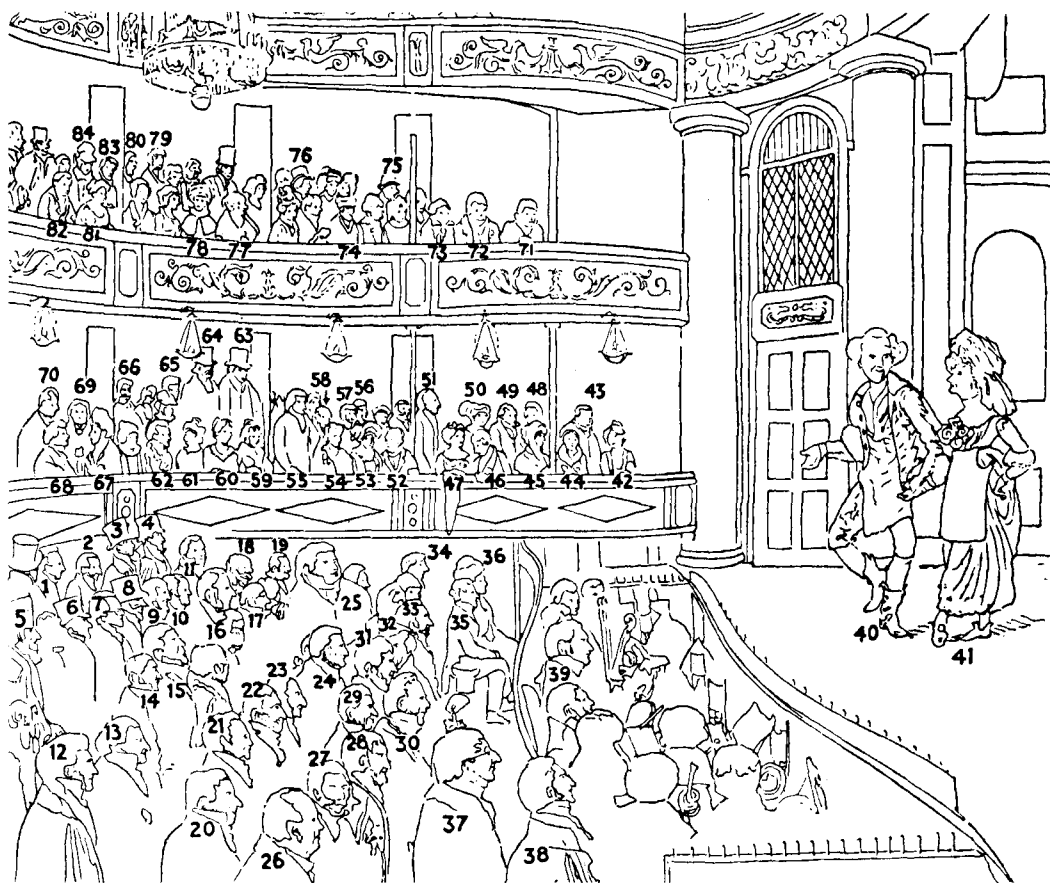
The town houses of David Sears, Nathaniel Prime, William B. Astor, William Bedlow Crosby, Peter Schermerhorn, Samuel Ward, Harrison Gray Otis, or Henry Brevoort would have been adjudged magnificent anywhere. If the lavish country residences of David Hosack, John C. Stevens, or James Gore King did not match the awesome size and cost of the duke of Northumberland's Alnwick Castle, neither did the mansions and estates of Northumberland's fellow aristocrats in England. Corps of servants, impressive libraries, elaborate furniture (often manufactured by Duncan Phyfe), sumptuous furnishings, stores of the finest wines, and expensive artworks filled the interiors of the homes of the American economic elite. In the warm weather months the rich retreated to the delights of the Rockaways and other ocean resorts or to the waters of Saratoga and regularly traveled to Havre in ships filled with their own kind, blessed with lavish accommodations, and in Hone's words, with "every day as good a table as the most fastidious gastronome could desire." Their lives at home during the workaday year were enlivened by a constant round of expensive parties, dazzling balls, extravagant fetes and excursions, binding more closely together the leading families both within and between the great cities. The birthday of the beautiful Elizabeth Willing in 1835 was celebrated on Long Island with a trotting match, won by Robert Goelet, a "splendid ball," and a cotillion. On a sparkling fall day later that year "in the beautiful Bay of Boston" aboard a pleasure schooner once owned by David Hosack and Robert Hone, among others, a fishing party of New York City's swells joined with the Brookses, Forbeses, Sturgises, and Bryants in sampling chowder and enjoying great quantities of champagne and fine madeira, to compensate for the mere handful of cod and haddock they had caught.

A fashionable marriage, such as the one uniting Charles A. Heckscher and the daughter of John G. Coster in late 1834, triggered off a round of balls and parties that left even inveterate pleasure seekers somewhat exhausted and a trifle dismayed at their extravagance. The great ball given by Henry Brevoort on February 28, 1840, excited widespread attention for its opulence, but much space in Hone's more than ten thousand pages is given over to description of many dozens of smaller scale but equally exclusive and splendid affairs. A *fête champêtre* of unusual elegance, such as that held at Thomas W. Ludlow's villa on the Hudson, in Phillipsburgh, near Yonkers, on June 26, 1845, could attract several hundred of the leading

"judges, lawyers, merchants, men of leisure and millionaires" of New York City to a "picnic" adorned with "every delicacy," fine band music for outdoors, waltzes, polkas, and cotillions within the house, the entire scene enlivened by the presence of several private yachts that circled the private steamer hired for the occasion by Ludlow to carry his guests to and from the festivities. Theater, Italian opera, soirees, and musical evenings also occupied the elite. Fastidious foreign visitors mocked the pretentiousness of the American urban elite's high life, but there could be no denying its expensiveness.

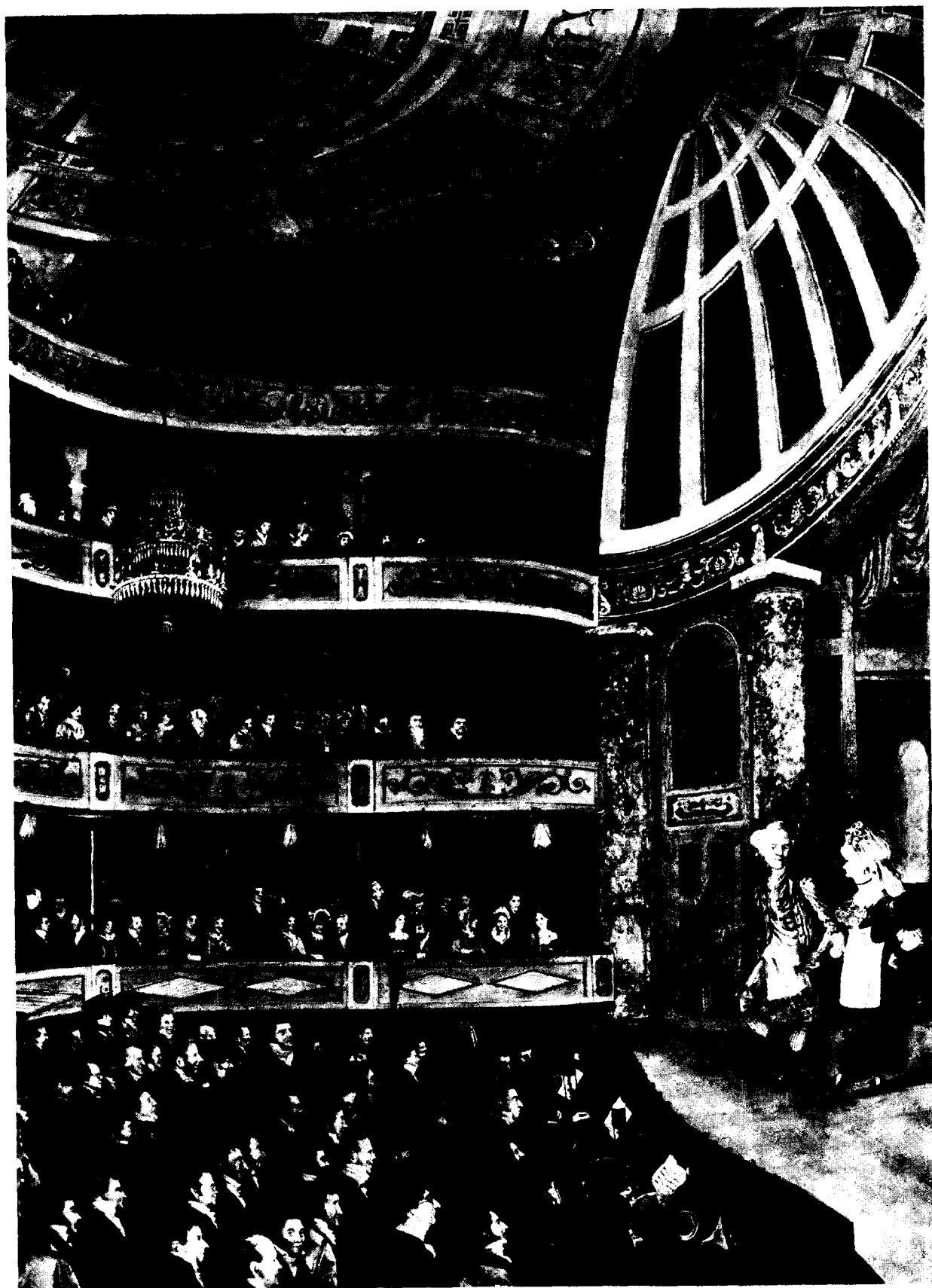
Those who referred to the paltriness of American fortunes doubtless had European wealth in mind. Unfortunately the wealth of the great European accumulators has not been and probably cannot be fixed with precision. For Old World riches as for New, informed estimates rather than precise sums have been offered by scholars. The estate of Nathan Mayer Rothschild, regarded as the wealthiest member of the great family and the richest man in Europe, "was generally assumed to be between £5,000,000 and £6,000,000," or roughly between \$21,100,000 and \$25,700,000, at the time of his death in 1836. The greatest figure in French finance in the early nineteenth century, Gabriel Julien Ouvrard, who alone in that nation dared think of rivaling the Rothschilds as international financier, could in 1820 "offer a prospective son-in-law a dowry of a million francs." At a high point in Ouvrard's career his worth has been estimated at slightly more than \$5,000,000.²⁹ The duke of Bedford's great estates in 1839 were worth about \$10,000,000, to judge from his indebtedness of £551,940, an annual net remittance for the preceding seven years of slightly over £100,000, and the small rate of profit typically earned on his landed property during the period. Only a handful of the three hundred or so families constituting England's landed aristocracy could approximate such wealth, with others such as the earl of Clarendon worth closer to \$375,000. Sir Francis Baring, whose fortune may have been matched or exceeded in England in the entire nineteenth century by Queen Victoria, Sir Robert Peel, the dukes of Bedford, Northumberland, Bridgewater, and Devonshire, and hardly any others, has been judged to have been worth slightly less than \$5,000,000 during the period, while his son Alexander had real-estate investments

²⁹ David Landes has advised me that it is impossible to determine precisely what Nathan Rothschild was worth, in view of the tangled nature of the great family's finances and the difficulties posed by Rothschild's will. Bertrand Gille's authoritative *Histoire de la Maison Rothschild, Des Origines à 1848* (Geneva, 1965) and *Histoire de la Maison Rothschild, 1848-1870* (Geneva, 1967) focuses on the family's corporate assets rather than the personal wealth of its members. For an informed estimate, see Cecil Roth, *The Magnificent Rothschilds* (London, 1939), 25-26. In arriving at the dollar value of the pound for the period I have followed Ralph Hidy's estimate, "calculating the dollar at 4 shillings and 8 pence," in "The House of Baring and the Second Bank of the United States, 1826-1836," *Pennsylvania Magazine of History and Biography*, 68 (1944): 270. Arthur-Lévy, *Un Grand Profiteur de Guerre: Gabriel Julien Ouvrard, 1770-1846* (Paris, 1929), 1; Otto Wolff, *Ouvrard, Speculator of Genius, 1770-1846* (London, 1962), xiv, 148, 187. The estimate of Ouvrard's wealth in dollars is drawn from Arthur-Lévy's work and from Wolff's judgment that "during most of Ouvrard's lifetime the rate of exchange was twenty-five francs to the pound." *Ouvrard*, xiv.



- | | | |
|---------------------------|---|---|
| 1 Nicholas G. Rutgers | 29 William Wilkes | 57 Dr. Hugh McLean |
| 2 William H. Robinson | 30 Charles Farquhar | 58 John Charnaud |
| 3 Charles G. Smedburg | 31 Pierre C. Van Wyck | 59 Miss Wilkes |
| 4 Robert G. L. De Peyster | 32 John Searles | 60 Mrs. C. D. Colden, <i>née</i> Wilkes |
| 5 Alexander Hosack | 33 John Berry | 61 Mrs. Robert Lenox |
| 6 Dr. John Neilson | 34 Robert Gillespie | 62 David S. Kennedy |
| 7 Dr. John W. Francis | 35 Edmund Wilkes | 63 John K. Beekman |
| 8 Castle Rotto | 36 Hamilton Wilkes | 64 Robert Lenox |
| 9 Thomas Bibby | 37 Captain Hill | 65 Cadwallader D. Colden |
| 10 John I. Boyd | 38 Robert Watts | 66 Swift Livingston |
| 11 Joseph Fowler | 39 George Gillingham | 67 Henry Brevoort |
| 12 Francis Barretto | 40 Charles Mathews | 68 James W. Gerard |
| 13 Gouverneur S. Bibby | 41 Miss Ellen A. Johnson | 69 James K. Paulding |
| 14 Thomas W. C. Moore | 42 Mrs. Gelston, <i>née</i> Jones | 70 Henry Carey |
| 15 James Allport | 43 Maltby Gelston | 71 Edward Price |
| 16 Walter Livingston | 44 Mrs. De Witt Clinton, <i>née</i> Jones | 72 Stephen Price |
| 17 Dr. John Watts | 45 Mrs. Newbold, <i>née</i> LeRoy | 73 Capt. John B. Nicholson |
| 18 James Farquhar | 46 William Bayard, Jr. | 74 Thomas Parsons |
| 19 James Mackey | 47 Miss Ogden | 75 Herman Le Roy, Jr. |
| 20 Henry N. Cruger | 48 Duncan P. Campbell | 76 William Le Roy |
| 21 John Lang | 49 Jacob H. LeRoy | 77 Herman Le Roy |
| 22 William Bell | 50 Mrs. Daniel Webster | 78 Mrs. Eliza Talbot |
| 23 Mordecai M. Noah | 51 William Bayard | 79 Alexander C. Hosack |
| 24 Hugh Maxwell | 52 Dr. Samuel L. Mitchell | 80 Robert Dyson |
| 25 William H. Maxwell | 53 Mrs. S. L. Mitchell | 81 Mrs. Samuel Jones |
| 26 James Seaton | 54 Mrs. James Fairlie | 82 Judge Samuel Jones |
| 27 Thomas F. Livingston | 55 Dr. David Hosack | 83 Dr. James Pendleton |
| 28 Andrew Drew | 56 James Watson | 84 Mrs. Pendleton, <i>née</i> Jones |

Fig. 1. Key to painting opposite. Courtesy of the *New-York Historical Society Quarterly*.



*Fig. 2. Prominent New Yorkers at the Park Theatre, Nov. 1822. Watercolor by John Searles.
Photograph courtesy of the New-York Historical Society.*

worth close to that sum. Lord Overstone, described by a modern authority as the chief figure of Britain's "new commercial aristocracy," had a fortune that at its height, decades after Tocqueville's visit here, has been estimated to have been about \$17,000,000.³⁰

While these were vast fortunes indeed, they appear to have been approximated by those of the wealthiest Americans. John Jacob Astor's wealth was close to Rothschild's. Dozens of American families commanded riches similar to what has been attributed to Ouvrard and the Barings. For that matter, Alexander Baring's fortune had been substantially abetted both by his marriage to William Bingham's eldest daughter and by his purchase of Bingham's vast Maine properties. European landed wealth of course was much older than American, a fact that possibly led contemporaries to question the extent as well as the vulgarity of the Yankee nouveaux riches. Herman Thorn, a fashionable New Yorker who, in marrying the niece of William Jauncey had also come into much of the latter's fortune, stayed in Paris during the 1830s. Thorn was said to have "lived in a style of princely splendor that eclipsed all rivalry, to the great astonishment of the French, who failed to comprehend where in America he had acquired such funds." In 1836 he was reported "to have spent \$8,000 on a single fancy dress ball in his Paris home [actually, a splendid palace]." According to Philip Hone his friend Thorn talked "about hundreds of thousands with the air of a man who has been born and brought up in the midst of gold, silver and precious stones."³¹ Yet although Thorn was a rich man, there were close to one hundred families in New York City alone whose wealth surpassed his. The notion that antebellum America lacked substantial fortunes is not borne out by the evidence, primarily, as will be noted, because of its faulty assumption concerning the alleged distribution of "resources to all the members of the community."

"IN AMERICA," WROTE TOCQUEVILLE, "most of the rich men were formerly poor."³² The idea that, in the words of Henry Clay, the wealthy and successful were "self-made men," came close to being an article of faith, so widely was it subscribed to by Americans during the era. The common man was constantly reminded that "the most exalted positions" or great wealth were accessible to men of humble origin, since in this country "merit and industry" rather than "exclusive privileges of birth" determined the course of one's career. The merchant prince, William E. Dodge, offered the estimate

³⁰ Spring, *English Landed Estate*, 35, 41; Thompson, *English Landed Society*, 25. Mr. Spring has suggested to me that multiplying the landed income of the great aristocrats by thirty and then subtracting their indebtedness yields a useful if rough approximation of their worth. Hidy, *House of Baring*, 40, 46; Thompson, *English Landed Society*, 39. Overstone had spent £1,670,000 in the purchase of estates, and by his death in 1883 he left £2,118,804 in stocks, shares, and other forms of personal property.

³¹ MS diary of Philip Hone, 10: 76; *New York Genealogical and Biographical Record* (hereafter NYGBR), 91 (1960): 91.

³² *Democracy in America*, 1: 54; see also 2: 138.

that seventy-five per cent of the era's wealthy men "had risen from comparatively small beginnings to their present position."³³ If few modern historians would commit themselves to a precise ratio, many have nevertheless agreed that a remarkable movement up the social and economic ladder characterized the second quarter of the nineteenth century.³⁴ We have evidently convinced our colleagues in sociology, including some of the leading students of social mobility and stratification, that for the Jacksonian period the facts are in: intergenerational economic and occupational mobility were the rule. Actually it is not the facts that are in but rather a continuing series of firmly stated generalizations that essentially do nothing more than assume that the facts would bear them out.³⁵

That Tocqueville in some instances was ready to spin his marvelous social theorems by reference more to logic than to pedestrian data is well known.³⁶

³³ See Dodge, *Old New York*, 38–40; Joseph Scoville, *The Old Merchants of New York* (New York, 1862–63), *passim*; Hunt, *Lives of Eminent Merchants*, *passim*; John W. Francis, *Old New York* (New York, 1866), ix; Calvin Colton, *Junius Tracts* (New York, 1844), 7: 15; Paul Goodman, "Ethics and Enterprise: The Values of a Boston Elite, 1800–1860," *American Quarterly*, 18 (1966): 440, 447; Moses Yale Beach, editorials in the *New York Sun*, Jan. 8, 11, 13, 18, 1845, and preface to *Wealth and Pedigree of the Wealthy Citizens of New York* (New York, 1842); *Memoirs of the Wealthy Citizens of Philadelphia*, the preface of which reported that "our wealthy citizens . . . pride themselves for having made their own money"; Charles Humphreys, *Philadelphia Merchant* (Philadelphia, n.d.); and Stephen N. Winslow, *Biographies of Successful Philadelphia Merchants* (Philadelphia, 1864), viii, 111, 137. The popularity of the belief during the antebellum period is discussed in John G. Cawalti, *Apostles of the Self-Made Man* (Chicago, 1965), and Irvin G. Wyllie, *The Self-Made Man in America: The Myth of Rags to Riches* (New York, 1966), 14–20.

³⁴ One well-known textbook states that "of the successful businessmen of the period before the Civil War, almost fifty per cent came from the lower social and economic strata of society." Dexter Perkins and Glyndon G. Van Deusen, *The United States of America: A History* (New York, 1968), 1: 446. For variations on the theme of the self-made Jacksonian rich, see Carl R. Fish, *The Rise of the Common Man: 1830 to 1850* (New York, 1927), 9; Berthoff, "American Social Structure," 499–500; Benson, *Concept of Jacksonian Democracy*, 165; Cunliffe, *The Nation Takes Shape*, 164; Morton Borden, *The American Profile* (Lexington, 1970), 114; Samuel Eliot Morison, *Oxford History of the American People* (New York, 1965), 475; Stuart Bruchey, *Roots of American Economic Growth*, 201, 207; and P. M. G. Harris, "The Social Origins of American Leaders: The Demographic Foundations," *Perspectives in American History*, 3 (1969): 218. According to Douglas Miller social lines hardened in New York after 1830, but earlier, "poor-boy-made-good examples abounded" in this socially mobile society. *Jacksonian Aristocracy*, 23, 59, 60, 181.

³⁵ See Seymour M. Lipset and Hans L. Zetterberg, "A Theory of Social Mobility," in Reinhard Bendix and Seymour Martin Lipset, eds., *Class, Status, and Power: Social Stratification in Comparative Perspective* (2d ed.; New York, 1966), 551. In arguing for a shift from descriptive to interpretive research that places greater stress on causes and consequences, the authors state that there is by now "enough descriptive material" on the "background of members of elite groups." See also Harold M. Hodges, *Social Stratification: Class in America* (Cambridge, 1964), 1; Leonard Reissman, *Class in American Society* (New York, 1959), 11, 243; and Sorokin, "American Millionaires and Multimillionaires," 635–36. Stephan Thernstrom's recent observation that "systematic studies of social mobility in nineteenth-century America are still woefully absent" is very much to the point. See Thernstrom, "Notes on the Historical Study of Social Mobility," in Don Karl Rowney and James Q. Graham, Jr., eds., *Quantitative History* (Homewood, 1969), 100. See also Stuart Blumin, "The Historical Study of Vertical Mobility," *Historical Methods Newsletter*, 1 (Sept. 1968): 1.

³⁶ For discussions of Tocqueville's predilection for the deductive method, see Lynn L. Marshall and Seymour Drescher, "American Historians and Tocqueville's Democracy," *Journal of American History*, 55 (1968): 517; Seymour Drescher, "Tocqueville's Two *Démocraties*," *Journal of the History of Ideas*, 25 (1964): 211–16; Edward T. Gargan, "Some Problems in Tocqueville Scholarship," *Mid-America*, 41 (1959): 3–26; Gargan, "Tocqueville and the Prob-

What is fascinating is the extent to which scholars, ordinarily skeptical of unverified observation, have relied on it in discussing the origins of the rich in the "age of the common man."

The social origins and parental status of wealthy citizens of Boston, Philadelphia, New York City, and Brooklyn have been investigated in order to test the belief that typically they were born poor. Information has been gathered on the several hundred wealthiest citizens in each of these great cities.³⁷ The evidence indicates that some of the best known among the wealthy citizens did in fact have the kind of background ascribed to them by the egalitarian thesis.

John Jacob Astor's story is perhaps improperly described as a rise from rags to riches. There is some question as to the precise wealth or status of his father. Whether the latter was a "very worthy" minor officeholder, as some described him, or a poor man devoted more to tippling than to industry, as he was depicted by others—for the moment I am prepared to regard the two judgments as contradictory—it seems fairly certain that the great merchant was indeed a self-made man of humble origin. The same can be said, with even more certainty, of his sometime partner, Cornelius Heeney, who migrated from Ireland apparently with less than a dollar in his pockets, to become one of the wealthiest residents in Brooklyn.³⁸ Lewis A. Godey, publisher of the popular *Ladies' Book*, John Grigg, and Joseph Sill were wealthy Philadelphians of humble beginnings, while Daniel P. Parker, Ebenezer Chadwick, John R. Adan, and the three Henshaw brothers in Boston also appear to have been of poor or humble birth, as were such New York eminences as Anson G. Phelps, Marshall O. Roberts, Gideon Lee, Saul Alley, and possibly the Lorillard brothers. Stephen Girard's claim that he, too, had been a destitute youth was evidently accepted by most contemporaries, although there is some doubt as to whether it was well founded.³⁹ Evidence is thus not lacking that some rich men had in fact been born poor. The most interesting feature of such evidence, however, is its uncommonness.

lem of Historical Prognosis," *AHR*, 68 (1962-63): 332-45; Jack Lively, *The Social and Political Thought of Alexis de Tocqueville* (Oxford, 1962); Marvin Zetterbaum, *Tocqueville and the Problem of Democracy* (Stanford, 1966); and George W. Pierson, *Tocqueville and Beaumont in America* (New York, 1938), 759-60.

³⁷ See the appendix for a discussion of the sources of family data.

³⁸ Porter, *John Jacob Astor*, 1: 4-5; John William Leonard, *History of the City of New York, 1609-1909* (New York, 1910), 2: 500; Thomas F. Meehan, "A Self-Effaced Philanthropist: Cornelius Heeney, 1754-1848," *Catholic Historical Review*, 4 (1918): 4.

³⁹ According to his most recent biographer, who had access to papers not available to earlier commentators, "the facts do not support the myth," propagated above all by Girard himself, as to his boyhood poverty. Wildes, *Lonely Midas*, 4, 5, 10-11, 319. Girard had received a substantial sum from his merchant father before emigrating to America as a junior officer, not a cabin boy. See *Memoirs and Auto-Biography of . . . the Wealthy Citizens of Philadelphia*, app., and Simpson, *Biography of Stephen Girard*, a book that, while it may err on Girard's earlier status, is a fascinating psychological study by a man who knew Girard at firsthand. More prosaic is John B. McMaster, *The Life and Times of Stephen Girard, Mariner and Merchant* (Philadelphia, 1918).



Fig. 3. South Street from Maiden Lane. 1828. An illustration of the lively waterfront activity that was the source of many New York City fortunes. Aquatint. William I. Bennett, artist and engraver. From the Edward W. C. Arnold Collection, lent by the Metropolitan Museum of Art. Photograph courtesy of Museum of the City of New York.



Fig. 4. Residence of William B. Crosby, wealthy New Yorker. Rutgers Place, New York. Photograph courtesy of the I. N. Phelps Stokes Collection, Prints Division, the New York Public Library, Astor, Lenox, and Tilden Foundations.



Fig. 5. Pleasure railway at John Stevens' Hoboken resort, "Elysian Fields." About 1833. Stevens was one of the elite of New York City. Colored lithograph by D. W. Kellogg & Co. Photograph courtesy of Museum of the City of New York.



Fig. 6. Mr. and Mrs. Ernest Fiedler and children at 38 Bond Street, New York. 1850. An example of the style of living enjoyed by a family that was not among the truly wealthy. Painting by F. Heinrich. Owned by Mrs. William Lothrop Rich. Photograph courtesy of Museum of the City of New York.



Fig. 7. Family and guests of the wealthy Dr. John Cheesman at a reception in 1840 at his home at 473 Broadway, New York City. Painting by August Edouart. Photograph courtesy of the New-York Historical Society.



Fig. 8. Fancy dress ball in Philadelphia. 1850. The type of event often attended by Philadelphia's elite. Lithograph by C. Harnisch. Photograph courtesy of the Historical Society of Pennsylvania.



Fig. 9. Residence of Harrison Gray Otis, one of the leading members of Boston's elite. Mount Vernon Street. Photograph courtesy of the Print Department, Boston Public Library.



Fig. 10. The "Rose" of Long Island. 1840. Julia Gardiner, of the wealthy Long Island family, is shown modeling in front of a commercial establishment in New York City. Colored lithograph by Alfred E. Baker. Photograph courtesy of Museum of the City of New York.



Fig. 11. Residence of David Sears, Beacon Street. 1849. (In the foreground: the Frog Pond, Boston Common.) Sears' mansion was regarded by many as the finest in the North if not in the nation. Photograph courtesy of the Bostonian Society, Old State House.



Fig. 12. Bowdoin Square. 1825. A residential center of elegant Bostonians. Pen and ink wash drawings. Photograph courtesy of the Bostonian Society, Old State House.

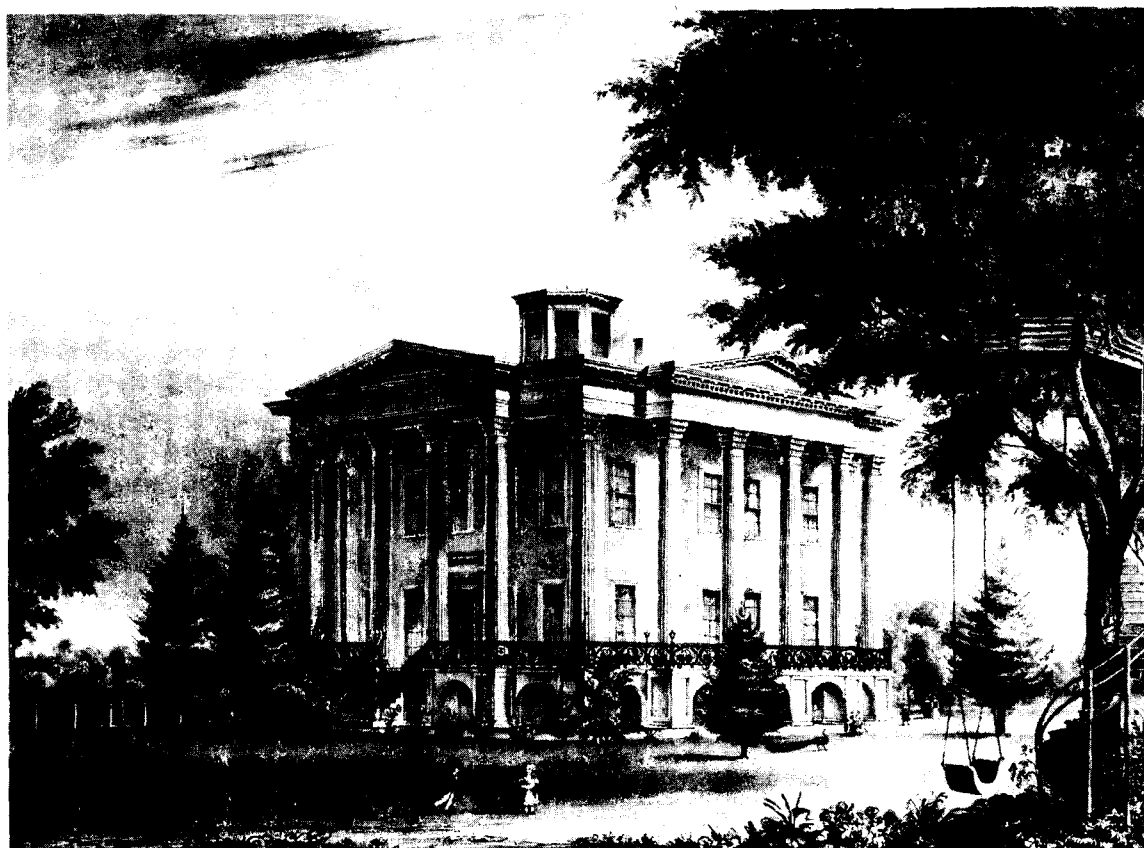


Fig. 13. Arlington House, 11th Avenue and 83d Street, Brooklyn. 1839. The owner, James Bennett, who described himself as "sole architect of his own house and of his own fortune," was not among Brooklyn's upper financial and social crust. Colored lithograph by J. H. Bufford. From the Edward W. C. Arnold Collection, lent by the Metropolitan Museum of Art. Photograph courtesy of Museum of the City of New York.

During the age of alleged social fluidity, the overwhelming majority of wealthy persons appears to have been descended of parents and families who combined affluence with high social status. The small number of these families that had been less than rich had typically been well to do.⁴⁰ Only about two per cent of the Jacksonian era's urban economic elite appear to have actually been born poor, with no more than about six per cent of middling social and economic status. Included in the middle are the families of Peter Cooper, William E. Dodge, Gerard Hallock, Joseph Sampson, Cornelius Vanderbilt, Moses Yale Beach, Peter Chardon Brooks, Amos and Abbot Lawrence, Thomas H. Perkins, George C. Shattuck, George Hall, Thomas Everitt, Jr., Samuel R. Johnson, Cyrus P. Smith, and Samuel Smith, all of whom appeared to have been both better off and of higher status occupations than the mechanics, cartmen, milkmen, and laborers who predominated in the cities. The middle category was composed of ministers, petty officials, professionals other than successful lawyers and doctors, shopkeepers, skilled artisans who doubled as small tradesmen, and independent or moderately prosperous farmers. The evidence for these generalizations, inevitably imperfect, requires explanation.

It was of course impossible to obtain reliable information on the family status of all persons, but fortunately abundant evidence exists on the backgrounds of most of the wealthiest persons in the great cities. Data were secured on ninety per cent of the more than one hundred New Yorkers who in 1828 were assessed for \$100,000 and upward, and in 1845 at \$250,000 or more; on eighty-five per cent of the more than one hundred Bostonians worth \$100,000 or better in 1833, and \$200,000 or more in 1848; and on about ninety per cent of the seventy-five Brooklynites who in 1841 were evaluated at \$60,000 or more. For Philadelphia, as was indicated earlier, the nature of the tax records does not permit them to be used to disclose the assessed total wealth of individuals. One can differentiate the "super rich" of that city from other rich or well-to-do persons only by accepting at face value the sums attributed in the anonymous *Memoirs and Auto-Biography of Some of the Wealthy Citizens of Philadelphia*. (Information was obtained on seventy per cent of the 365 persons each claimed by the *Memoirs* to be worth \$100,000 or more.) The pattern of the social backgrounds of the urban rich was strikingly similar for all the Northeastern cities. About ninety-five per cent of New York City's one hundred wealthiest persons were born into families of wealth or high status and occupation; three per cent came of "middling" background; only two per cent were born poor. As small a portion of Boston's one hundred wealthiest citizens started humble, with perhaps six per cent originating from middling families. Philadelphia's statis-

⁴⁰ Compare Jackson Main's treatment of this issue in *Social Structure of Revolutionary America*, 184. Main concludes that the dozen or so wealthiest Virginians of 1787 whose families were "well-to-do though not wealthy," were not self-made and that those who "inherited part of their wealth cannot really be considered mobile." (*Italics mine.*)

tics differ from Boston's only in that four per cent of the former city's 365 richest citizens were born into families of middling status; two per cent of her wealthiest citizens started poor. Cornelius Heeney and John Dikeman were the only wealthy Brooklynites of truly humble origins, with sixteen per cent born into middling status, and the remaining eighty-one per cent of wealthy or high-status families.

Evidence was not as freely available for the "lesser rich" of the great cities. Data were obtained on about seventy per cent of the more than 450 New Yorkers assessed at between \$25,000 and \$100,000 in 1828, and for sixty-three per cent of the 950 New Yorkers who in 1845 were worth between \$45,000 and \$250,000; on close to sixty-five per cent of the 260 Bostonians evaluated at between \$50,000 and \$200,000, in 1833, and on the same percentage of the four hundred Bostonians similarly assessed in 1848; and sixty-three per cent of the one hundred Brooklynites assessed in 1841 at \$30,000 to \$60,000. It is of course possible that the backgrounds of the "missing persons" were unlike those of the much larger number of persons for whom information was obtained. It could be argued that the omissions concern less eminent persons, whose families probably were not as wealthy or of as high status as the families whose careers and records are better publicized.⁴¹ Yet a significant feature of the evidence is its disclosure that there appeared to be no difference in the patterns of social origin among the "lesser wealthy" as against the "super rich"; or in the patterns of family background of the relatively little known or unknown rich for whom information was obtained as against the eminent rich.

Many of the era's richest men, while born into relative affluence, managed to carve out fortunes that far surpassed their original inheritances. Such persons were self-made only in a special sense, their careers hardly illustrating what publicists of the era meant by that term. That the children of high-status parents, living in an age of dynamic growth, convert their original advantages into fortunes of unprecedented scope is—as Jackson Turner Main has noted in another context—hardly a sign of social mobility. A family whose adult heads for four or five generations were among the economic elite of their city or community cannot be said to have experienced upward social movement because their always inordinate wealth kept increasing.

The rags-to-riches ideology had so penetrated American thought during the era that publishers whose own compilations contradicted the thesis could manage to convince themselves that it was nevertheless true. Freeman Hunt, devoted and enthusiastic admirer of America's merchants, whom he extolled in his charming *Merchants' Magazine*, could somehow describe Walter Restored Jones of the old, eminent, and wealthy family of Cold

⁴¹ For the argument made in a different context, that persons for whom family data are lacking were not necessarily of lower status than those for whom the data exist, see Seymour Martin Lipset and Reinhard Bendix, *Social Mobility in Industrial Society* (Berkeley, 1963), 125–26.

Spring, Long Island, truly one of fortune's favorites, as a "self-taught and self-made man."⁴² Popular ideology notwithstanding, the era of the common man was remarkable above all for how few rich men were in fact descended of common folk.

When it is compared with earlier periods in American history, the age of egalitarianism appears to have been an age of increasing social rigidity. According to a recent study of seventeenth-century Salem, while "some members of the rapidly emerging elite began their careers propertyless and benefited from the opportunities for investment . . . more often they emigrated with considerable wealth which was further augmented by fortuitous investment."⁴³ Jackson Main has concluded that there was "remarkable opportunity for the man of modest property to become rich" in the late eighteenth century. Main's admittedly imperfect and partial data on the three greatest cities of the Northeast are of special interest. He finds that about one-third of the sixty wealthiest Bostonians of 1771 had started with little or nothing; in 1789 only one-half of a small number of the city's wealthiest merchants had been born into "wealthy or well-to-do families," with the rest scattered among middling or lower status occupations. Of a group of one hundred wealthy Philadelphians, about "one third had made their own fortunes." He found that "between one third and two fifths of the merchants in pre-Revolutionary New York City [actually, members of the Chamber of Commerce] were self-made men," while in the years immediately after the Revolution the high "mobility rate" actually went up: "probably sixty per cent at the least [of a number of merchants in 1786] were self-made men," and in 1791 fifty per cent of the wealthiest citizens of the east ward had risen from humble origins.⁴⁴

A recent study of post-Revolutionary New York City concludes that for the period ending in 1815 "the evidence of upward social mobility is marked. Almost two thirds of the attorneys and merchants in public office had risen above the occupational level of their fathers who were mechanics or farmers."⁴⁵ The evidence on the earlier period, scattered and partial

⁴² Hunt, *Lives of Eminent Merchants*, 1: 428. Jones was an innovative figure in the history of antebellum commercial and marine insurance, yet he owed his start not to any sudden blooming of youthful commercial genius on his part but to his name, his status, and his own family's involvement in the insurance business. As for the self-delusion of the publishers of the Wealthy Citizens ratings, even when their unsubstantiated biographical sketches are taken at face value, men born rich far outnumbered men born poor. Most of these booklets offered slim biographical material of any sort.

⁴³ Donald Warner Koch, "Income Distribution and Political Structure in Seventeenth-Century Salem, Massachusetts," *Essex Institute Historical Collections*, 105 (1969): 51.

⁴⁴ Main, *Social Structure of Revolutionary America*, 163, 189-93.

⁴⁵ Edmund Willis, "Social Origins of Political Leadership in New York City from the Revolution to 1815" (Ph.D. dissertation, University of California, Berkeley, 1967), 171. For a contrasting viewpoint concerning mobility in the Revolutionary era, see Robert A. East, *Business Enterprise in the American Revolutionary Era* (New York, 1938), 213, and Kenneth W. Porter, ed., *The Jacksons and the Lees: Two Generations of Massachusetts Merchants, 1765-1844* (Cambridge, Mass., 1937), 3-150, which stresses the "strong tendency" for businessmen to beget businessmen. Also useful are Nash, "The Philadelphia Bench and Bar," 217-19; McCoy, "Political Affiliations of American Economic Elites," which shows that almost all of Wayne County's successful businessmen were well to do earlier; and for the post-Civil War decades, Clyde

though it may be, suggests that a substantial upward economic mobility that had characterized Northeastern urban life came to a halt during the so-called age of the common man. The self-made man, recently shown by William Miller and his students to have been more fantasy than fact in the post-Civil War decades,⁴⁶ was evidently a creature of the imagination a generation earlier, at the very time that the great Henry Clay was asserting the phantom's corporeality and ubiquitousness.

A RELATED BELIEF holds that the second quarter of the nineteenth century was "a highly speculative age in which fortunes were made and lost overnight, in which men rose and fell . . . with dexterous agility."⁴⁷ Tocqueville believed that fortunes here were both scanty and "insecure," wealth ostensibly circulating with "inconceivable rapidity." Contemporary American merchants insisted that theirs was the most precarious of callings, incapable of attaining the "security which accompanied the more pedestrian occupations."⁴⁸ True, the eminent Philip Hone had noted the resiliency of businessmen: "Throw down our merchants ever so flat [and] they roll over once and spring to their feet again"; but this optimistic judgment was confided to his private diary.⁴⁹ The prevailing view was that the pre-industrial decades were characterized by great intragenerational economic mobility.⁵⁰ It has

Griffin, "Making It in America: Social Mobility in Mid-Nineteenth Century Poughkeepsie," *New York History*, 51 (1970): 479-500.

⁴⁶ William Miller, "American Historians and the Business Elite," in William Miller, ed., *Men in Business: Essays on the Historical Role of the Entrepreneur* (New York, 1962), 311-28, examines the family status and backgrounds of 200 business leaders of the late nineteenth and early twentieth centuries; Frances W. Gregory and Irene D. Neu, "The American Industrial Elite in the 1870's: Their Social Origins," *ibid.*, 193-211, studies the backgrounds of 300 leaders from textiles, steel, and railroads. Discovering that "poor immigrant boys and poor farm boys actually made up no more than three per cent of the business leaders" he studied, Miller concludes his essay with the statement that poor boys "who become business leaders have always been more conspicuous in American history books than in American history." Herbert G. Gutman, on the other hand, has recently shown that "the most successful Paterson iron, locomotive, and machinery manufacturers" of the late nineteenth century were with few exceptions self-made men who had come to the small New Jersey city as ironworkers or craftsmen who "opened small shops or factories of their own," after completing their apprenticeships. Gutman, "The Reality of the Rags-to-Riches 'Myth': The Case of the Paterson, New Jersey, Locomotive, Iron, and Machinery Manufacturers, 1830-1880," in Thernstrom and Sennett, *Nineteenth-Century Cities*, 98-124. Gutman notes that the biographical material he has drawn from several directories and books refers "not [to] a sample" but to an entire group. The group, however, includes only about thirty men, and it is not clear how rich they were. But his study suggests the value in further examinations of the backgrounds of small businessmen and skilled artisans in other communities. It may be that in the early phases of certain manufacturing industries success required a know-how that came only with working in the craft.

⁴⁷ Bruchey, *Roots of American Economic Growth*, 206.

⁴⁸ Robert G. Albion, "Commercial Fortunes in New York: A Study in the History of the Port of New York About 1850," *New York History*, 16 (1935): 167-68; Tocqueville, *Democracy in America*, 1: 53, 2: 105, 234, 250-51.

⁴⁹ MS diary of Philip Hone, 23: 165.

⁵⁰ For modern variations on this view, see Marcus Cunliffe, *The Nation Takes Shape*, 169; Berthoff, "The American Social Order," 499-500; Ward, "The Age of the Common Man," 86; Potter, *People of Plenty*, 95; Meyers, *The Jacksonian Persuasion*, 46; Degler, *Out of Our Past*, 144.

recently been shown, however, that antebellum Philadelphia witnessed slight movement up and down the occupational ladder or to and from residential districts of clearly differentiated wealth and status.⁵¹ Another recent study examines the changing economic circumstances of thousands of Bostonians and New York City residents of different wealth levels over the course of a generation.⁵² Some generalizations, drawn from its detailed findings, follow.

The richest Bostonians of the early Jacksonian era were invariably among the very richest Bostonians late in the period. Very few persons of the upper-middle wealth level—only seven per cent of that group—moved upward into the wealthy category whose members were each assessed for \$50,000 or more. The extent of an individual's early wealth was the major factor determining whether he would be among the rich later. Absolute increases in wealth of any sort followed the rule: the greater an individual's initial wealth, the greater the amount by which it was augmented. A companion rule was that the greater one's original riches, the more likely was he to enjoy an increase. Since the population by mid-century had increased substantially in two decades the ranks of the later rich necessarily had to be filled by many persons who earlier were not among the wealthy. More often than not these newly rich taxpayers were younger members of old families, since fewer than ten per cent of the later group of Boston's rich were new men. Not one member of the \$100,000 group of mid-century who had paid taxes earlier had paid them on less than the \$20,000 owned by the wealthiest two per cent of the Boston population. Since many contemporaries claimed that the careers of successful merchants followed an erratic course in this kaleidoscopic economy, changes over short-run periods were also investigated to determine whether persons who started and ended the race strong may have lagged in between. They did not. In Boston "few new great families sprang up while fewer still fell away" during the era.

New York City's statistics for the period were not an exact replica of the Boston evidence. Since New York was richer all categories of wealth from the upper middle on up experienced greater gains in absolute wealth than did their counterparts in Boston. For the rest the general pattern was remarkably similar for the two great cities. Between the period of Andrew Jackson's

⁵¹ Blumin, "Mobility in a Nineteenth-Century American City," 90, 109, 134–52. Winslow long ago wrote that financial crises "touched Philadelphia with gentle wings." In Philadelphia there was "more real, solid, enduring wealth than in any [other] city in the Union"; *Biographies of Successful Philadelphia Merchants*, vii. For valuable discussions of the relationship between occupational change and social mobility, see Ely Chinoy, "Social Mobility Trends in the United States," *American Sociological Review*, 20 (1955): 180–86; Paul K. Hatt, "Occupation and Social Stratification," *American Journal of Sociology*, 55 (1950): 534; Gerhard E. Lenski, "Trends in Inter-Generational Occupational Mobility in the United States," *American Sociological Review*, 23 (1958): 514–23; Elton F. Jackson and Harry J. Crockett, Jr., "Occupational Mobility in the United States: A Point Estimate and Trend Comparison," *ibid.*, 29 (1964): 5–15; and Otis Dudley Duncan, "The Trend of Occupational Mobility in the United States," *ibid.*, 30 (1965): 491–98.

⁵² Edward Pessen, "Did Fortunes Rise and Fall Mercurially in Antebellum America? The Tale of Two Cities: Boston and New York," *Journal of Social History*, 5 (1971): 339–59.

first election to the presidency and his death not quite two decades later only one of New York City's fifty richest persons fell from the class of the rich, and even he barely failed to qualify. As in Boston the few New Yorkers who rose from the upper-middle wealth level to the rich during the course of the era "were more often than not from families of great wealth." The "newcomers" were younger members of the great Hendricks, Jones, Lenox, Lorillard, Barclay, Cruger, Grinnell, Bronson, Grosvenor, Hone, Lawrence, Post, Murray, Storm, Ward, Remsen, Schieffelin, and Van Rensselaer families or of "others of like distinction." About seventy-five per cent of the New York City families constituting the plutocracy of the so-called industrial era of the mid-1850s were families that comprised the elite of the merchant-capitalist era of a generation earlier.⁵³

Brooklyn assessment data exist for 1810 and 1841. If the earlier date falls before what even the most flexible classifications would consider the "Jacksonian era," that fact hardly detracts from its value. If anything the earlier starting point permits those so inclined to draw conclusions about economic fluidity between the "Jeffersonian" and "Jacksonian" periods. Brooklyn's wealthiest families of the early nineteenth century remained among the wealthiest families of the 1840s. Only one of the truly rich of 1810 fell by the wayside and not because of poverty but because of death. In Brooklyn, as in its mighty neighbor, riches achieved by early in the nineteenth century appeared to be the surest guarantee to the possession of wealth a generation later. The many wealthy persons of 1841 who were relative newcomers to the city had achieved their success almost without exception "as a result of a great boost given them at birth by wealthy or comfortably situated parents or relatives."⁵⁴

The pursuit of wealth in Jacksonian America was marked not by fluidity but by stability if not rigidity. Great fortunes earlier accumulated held their own through all manner of vicissitudes.⁵⁵ The tax records indicate that the panic of 1837 appeared to have no effect on the minuscule rate by which

⁵³ Close to fifty of the sixty merchants who contributed \$100 each to equip the Seventh Regiment of New York City in 1861 were of the prominent families of an earlier generation. Among the donors were members of the Grinnell, DeForest, Emmet, Wetmore, Blatchford, Minturn, Haggerty, Griswold, Fish, Manice, Blunt, Titus, Knapp, Stout, Stewart, Brown, Alsop, Aspinwall, Chauncey, Bronson, Prime, Coster, Aymar, Oothout, Ward, and Swan families. Martha Lamb and Mrs. Burton Harrison, *History of the City of New York* (New York, 1877, 1880, 1896), 3: 773. For evidence that the New York City upper crust of the late nineteenth and early twentieth centuries continued to consist primarily of descendants of the elite of the Tocqueville era, see the listings of the modern groups in Nathaniel Burt, *First Families: The Making of an American Aristocracy* (Boston, 1970), 285, and Cleveland Amory, *Who Killed Society?* (New York, 1960), 119-20, 132-33.

⁵⁴ This and the other conclusions about fluidity in nineteenth-century Brooklyn are developed statistically in Pessen, "A Social and Economic Portrait of Jacksonian Brooklyn."

⁵⁵ The generalization may also apply to the eighteenth century. Seventy-five per cent of the two dozen families assessed for the greatest wealth in New York City in 1674 were still among the city's wealthiest families in 1828. About ninety per cent of New York City's "Successful Business Men" of 1786 as well as the elite personages who appeared on Mrs. John Jay's exclusive "Dinner and Supper List for 1787 and '8," were still among the elite of wealth two generations later. See *Valentine's Manuals* (New York, 1841-42) and James Grant Wilson, *Memorial History of the City of New York* (New York, 1892-93), 1: 362; 3: 87-101.

the mighty fell or the puny rose during the years surrounding that economic convulsion. The Boston tax records disclose that of the owners of the modest property evaluated at between \$5,000 and \$7,000 prior to the panic of 1837, less than one per cent became significantly wealthier in its wake, while slightly more than one-third were badly hurt by the cataclysm or compelled to leave the city. In contrast, only two of the nearly one hundred Bostonians worth \$100,000 or more each suffered substantial losses, while about twenty-three per cent of them enjoyed gains of \$20,000 or better in the immediate aftermath of the financial crisis.

That the rich typically were well born and held on to or increased their wealth does not prove that there was no social mobility during the era. The vast and swelling sociological literature on the related topics of "vertical mobility" and social stratification makes clear that the concept of social mobility is a most complex one, not least because it involves the intangible of status. As has recently been pointed out, "there are a host of different ways of measuring mobility. And mobility has many varied contours."⁵⁶ No last word can ever be said concerning a subject so elusive and for which the data are so often imperfect.⁵⁷

If, as Ralf Dahrendorf has written, "the concept of social mobility is too general to be useful," there is much to be said for dealing with specific aspects of it rather than with the concept as a whole. All of which is to say that if no data can measure the immeasurable—social mobility in general—the evidence pointing to the upper-class backgrounds of the Jacksonian era's elite and the tenacity with which they held on to their wealth undermines two of the main supports of the long-popular belief in antebellum mobility.

⁵⁶ Thomas Fox and S. M. Miller, "Occupational Stratification and Mobility," in Bendix and Lipset, *Class, Status, and Power*, 581. Almost fifteen years ago Bernard Barber noted the vastness of the relevant literature in his *Social Stratification* (New York, 1957), as did Raymond W. Mack, Linton Freeman, and Seymour Yellin, in *Social Mobility: Thirty Years of Research and Theory* (Syracuse, 1957). As even a hasty glance at the articles in the *American Sociological Review* of the past decade would indicate, the pace of publication on the topic has quickened. Allusions to the complexity and variety of definitions of social mobility, as well as to the many ingredients subsumed under the topic, can be found in Sorokin, *Social Mobility* (New York, 1927), 13; Bendix and Lipset, *Class, Status, and Power*, 1, 6, 112; D. V. Glass, ed., *Social Mobility in Britain* (London, 1967), introduction, 5; Ralf Dahrendorf, *Class and Class Conflict in Industrial Society* (Stanford, 1959), 220; R. Mukherjee and J. R. Hall, "A Note on the Analysis of Social Data," in Glass, *Social Mobility in Britain*, 218; Thernstrom, "Notes on the Historical Study of Social Mobility," 107; and Charles F. Westoff, Marvin Bressler, and Philip C. Sagi, "The Concept of Social Mobility: An Empirical Inquiry," *American Sociological Review*, 25 (1960): 375-85, whose list of twenty-two ingredients of mobility is admittedly not comprehensive.

⁵⁷ The kind of evidence that enabled Stephan Thernstrom to study the occupational movement of working-class children in Newburyport, 1850-80, is not always available. See Thernstrom, *Poverty and Progress* (Cambridge, Mass., 1964); see also Harris, "Social Origins of American Leaders," 161. Valuable criticisms of the methodology employed in earlier mobility studies are offered in Natalie Rogoff, *Recent Trends in Occupational Mobility* (Glencoe, 1953), 13; Otis Dudley Duncan, "Methodological Issues in the Analysis of Social Mobility," in Neil J. Smelser and S. M. Lipset, eds., *Social Structure and Mobility in Economic Development* (Chicago, 1966), 51-97, particularly p. 52; Saburo Yosuda, "A Methodological Inquiry into Social Mobility," *American Sociological Review*, 29 (1964): 16-23; and Gosta Carlsson, *Social Mobility and Class Structure* (Lund, 1958), chs. 5, 6.

THE FINAL QUESTION to be considered in this discussion concerns the distribution of wealth in the age of equality. Did the rich command an inordinate share and did it increase or dwindle during the period?

A keystone of the egalitarian intellectual structure is the belief that, in Tocqueville's words, a "general equality of condition" prevailed here. A perfect equality was of course out of the question. Pariah ethnic groups and hordes of unwashed new immigrants obviously were not in on the feast. But that the cornucopia was almost equally available to most others, like other elements in the egalitarian canon, remains a living belief.⁵⁸ Even a modern scholar who dissents from the consensus, finding that in New York State "heavy immigration and industrialization" after 1830 widened the gulf between the classes, concedes that earlier "there did not appear to be any contradiction between the notion of equality of opportunity and a general equality of condition."⁵⁹ By this version, the prefatory age, or what economic historians have called the age of merchant capitalism, was indeed an age of equality. The comprehensive evidence I have gathered on what almost every urbanite was worth early and late in the era makes possible an empirical test of this thesis. The fact that other scholars have performed similar quantitative studies of the distribution of wealth for earlier periods and that useful evidence exists for the Civil War years and later permits us to compare the degree of equality in the "age of egalitarianism" with that of other periods in American history.

During the colonial era wealth had become more unequally distributed with the passing years. This at least is the burden of the modern studies of scattered towns and villages. In Chester County, Pennsylvania, the richest ten per cent of the population owned slightly less than one-quarter of the wealth in 1693. Over the course of the next century their share increased to slightly under two-fifths, from 23.8 per cent to 38.3 per cent of the total, at the same time as the proportion owned by the poorest three-fifths of the population declined from 38.5 per cent to 17.6 per cent. Wealth was distributed less equally in commercial or seaport towns, and the tempo of increasing maldistribution was swifter in such communities. Where the wealthiest five per cent of property owners in Salem owned about one-fifth of its wealth during the quarter century before 1660, by 1681 their portion had risen to about one-half of the prospering Massachusetts town's total. In colonial Boston the wealthiest one per cent of the population owned about one-tenth, the richest five per cent about one-quarter, and the upper fifteen per cent about one-half of the city's real and personal estate in 1687. By 1771 the

⁵⁸ Marvin Meyers sees a "narrowed spread of property differences, with a heavy concentration in the middle range," *Jacksonian Persuasion*, 47; David M. Potter, an abundance that more than any other factor shaped American ways and values, *People of Plenty, passim*; and Carl R. Fish believed that "there was so close an approximation to economic equality to match the political that effort and ability could raise anyone to the top," *Rise of the Common Man*, 9.

⁵⁹ Miller, *Jacksonian Aristocracy*, x.

wealthiest three per cent of Boston's population held slightly over one-third of the city's wealth, while the upper ten per cent owned about fifty-five per cent of the property of a Boston community that had become "more stratified and unequal." Precisely the same share was owned by Philadelphia's upper tenth of "potential wealthholders" in 1774. On the eve of the Revolution, the richest ten per cent of Northerners owned about forty-five per cent of the wealth, a figure slightly greater than the amount of net worth controlled by the richest tenth of the middle colonies for 1774.⁶⁰ A less detailed comparison of New York City between 1789 and 1815 notes that the wealthiest thirty per cent of the city's fourth ward increased slightly their share, from seventy-one to seventy-six per cent of the community's wealth during that quarter of a century.⁶¹

Was the inegalitarian trend reversed in the nineteenth century? During the "age of equality" wealth in Boston became more unequally distributed than ever before.⁶² On the eve of the Revolution Boston's richest tenth had held slightly more than one-half of the city's wealth. Very little change evidently occurred over the course of the next half century, according to a local census report, whose table of Boston's tax payments for 1820 indicated that the upper one per cent controlled about one-sixth of the city's wealth, while the richest tenth continued to own the slightly more than one-half they had held in 1771.⁶³ Significant changes occurred over the following decade, since by 1833 the pattern of distribution had been sharply altered. The inegalitarian trend accelerated during the next fifteen years. (See tables 1 and 2.)

Actually the richest Bostonians owned a larger share of their city's

⁶⁰ The figures for Philadelphia are taken from Alice Hanson Jones, "Wealth Distribution in the American Middle Colonies in the Third Quarter of the Eighteenth Century," paper read at the annual meeting of the Organization of American Historians, Apr. 17, 1971, in New Orleans, as are the figures for the middle colonies in 1774. The figures for the North on the eve of the Revolution are from Main, *Social Structure of Revolutionary America*, 42. The other figures are from Donald Warner Koch, "Income Distribution and Political Structure in Seventeenth-Century Salem," 54, 58, 59, 63; James T. Lemon and Gary B. Nash, "The Distribution of Wealth in Eighteenth Century America: A Century of Changes in Chester County, Pennsylvania, 1693-1802," *Journal of Social History*, 2 (1968): 13; and James Henretta, "Economic Development and Social Structure in Colonial Boston," *William and Mary Quarterly*, 22 (1965): 79, 80, 82, 87, 89, 92.

⁶¹ Willis, "Social Origins of Political Leadership in New York City," 110. For the distribution of wealth in a small Connecticut town, see Charles S. Grant, *Democracy in the Connecticut Frontier Town of Kent* (New York, 1961), 34, 96-97.

⁶² For Boston, New York City, and Brooklyn the distribution of wealth was arrived at by the following process: the total assessed wealth of the city, corporate and noncorporate, was determined for a given year. Taxpayers were grouped according to the level or category of their wealth. The assessed wealth of all members of each category was then added up in order to determine the percentage of the city's total (noncorporate) wealth they owned. In determining the percentage of the city's population represented by the persons in a given wealth category, the denominator used was the number of families in the city rather than the total population. Not to have done so would have suggested a far more drastic inequality than was actually the case, since it would have converted rich men's wives, children, and other dependents into so many "propertyless" individuals.

⁶³ My generalizations are based on computations performed on the table in Shattuck, *Census of Boston for the Year 1845*, 95.

TABLE 1. Distribution of Wealth in Boston in 1833

<i>Level of Wealth</i>	<i>Percentage of Population</i>	<i>Approximate Total Wealth Owned^a</i>	<i>Percentage Non-corporate Wealth</i>
\$75,000 or more	1%	\$19,439,000	33%
\$30,000 to \$75,000	3%	\$15,000,000	26%
\$ 5,000 to \$30,000	10%	\$16,047,400	27%
Under \$5,000	86%	\$ 8,331,000	14%

^a In 1833 Boston wealth was listed at one-half its assessed value. In this table, therefore, the sums are doubled.

wealth than tables 1 and 2 indicate. In Boston as elsewhere a small number of rich men appeared to own most of the capital of their city's great financial institutions. A careful check reveals that Boston's wealthiest merchants and businessmen were the officers and directors, and therefore the major shareholders, of the city's fifty largest banks and insurance companies. The disparity between the actual proportion of Boston's entire wealth owned by the elite and the share indicated in the tables 1 and 2 (based on the assessments) is not as great, it will be shown, as were the disparities for New York City and Brooklyn. Boston banks and insurance companies were assessed only on their real estate, a relatively small component of the city's wealth. Private individuals who owned corporate wealth were evidently assessed for their holdings; in sharp contrast to New York City and Brooklyn, therefore, taxpayers in Boston were assessed for personal property almost equal in value to their real estate. The fact, however, that in Boston as elsewhere the undervaluation of all property favored the rich above all, since they had the most to hide, is the chief assurance that actual wealth was more unequally distributed than was assessed wealth.

A contemporary yeasayer wrote that in Jacksonian New York City, "wealth [was] universally diffused."⁶⁴ Even the normally optimistic Philip Hone disagreed, noting disconsolately that his beloved New York City late in the era had "arrived at the [unhappy] state of society to be found in the large cities of Europe," in which "the two extremes of costly luxury in living,

TABLE 2. Distribution of Wealth in Boston in 1848

<i>Level of Wealth</i>	<i>Percentage of Population</i>	<i>Approximate Total Wealth Owned</i>	<i>Percentage Non-corporate Wealth</i>
\$90,000 or more	1%	\$47,778,500	37%
\$35,000 to \$90,000	3%	\$34,781,800	27%
\$ 4,000 to \$35,000	15%	\$40,636,400	32%
Under \$4,000	81%	\$ 6,000,000	4%

⁶⁴ Stephen Girard, *The Merchants' Sketch Book and Guide to New York City* (New York, 1844), 6. Whoever this author may have been, he was not the great Philadelphia merchant, who was dead thirteen years by the date of publication of this pamphlet.

expensive establishments and improvident waste are presented in daily and hourly contrast with squalid misery and hopeless destitution."⁶⁵ The evidence bears out Hone's gloomy assessment.

In the year of Andrew Jackson's election to the presidency the wealthiest four per cent of the population of New York City, in owning almost half the wealth, controlled a larger proportion of the city's wealth than the richest ten per cent had evidently owned in the urban Northeast as a whole a half century earlier. By 1845 the disparities had sharply increased.

To judge from the New York City evidence, the rate by which the rich got proportionately richer became much more rapid during the nineteenth century than it had been during the seventeenth or eighteenth. As for the city's inequality in 1828 and 1845, its full extent is not disclosed in the assessment figures for these years.

A committee of the Common Council had reported that it was persons of "very extensive capital" who paid taxes on personal property "far less in proportion than those in moderate and low circumstances." In view of the way in which the underassessments of all wealth masked the true wealth of the rich above all, it is clear that the proportion of the city's wealth owned by a small upper crust was greater than the figures indicate. If, as contemporary municipal officials believed, the richest of the rich owned most of the hidden personal wealth, the proportion of the city's total wealth they controlled goes up by a figure dependent on the percentage of the undisclosed wealth that is attributed to them. On the basis that the personal property of the rich equaled the worth of their real estate, and that the wealthiest four per cent owned about nine-tenths of New York City's unassessed personal property, the upper one per cent would have owned about thirty-five per cent and the next wealthiest three per cent about twenty-two per cent of all noncorporate wealth in 1828. In 1845, by this reckoning, the richest one per cent would have owned about forty-seven per cent, while the next wealthiest three per cent would have held an additional thirty-two per cent of the city's noncorporate wealth. Nor do these estimates take into account the likelihood that the actual worth of the real property owned by the largest wealth holders was also undervalued. Perhaps the latter distortion can be

TABLE 3. Distribution of Wealth in New York City in 1828

<i>Level of Wealth</i>	<i>Percentage of Population</i>	<i>Approximate Non-corporate Wealth Owned^a</i>	<i>Percentage Non-corporate Wealth</i>
\$35,000 or more	1%	\$25,517,000	29%
\$ 7,500 to \$35,000	3%	\$17,520,000	20%

^a The figures used here for the city's total wealth are slightly less than the figures given in Thomas F. Gordon's *Gazeteer of the State of New York* (New York, 1836), because I have excluded the assessments on partnerships.

⁶⁵ MS diary of Philip Hone, 24: 408.

TABLE 4. Distribution of Wealth in New York City in 1845

<i>Level of Wealth</i>	<i>Percentage of Population</i>	<i>Approximate Non-corporate Wealth Owned^a</i>	<i>Percentage Non-corporate Wealth</i>
\$55,000 or more	1%	\$85,804,000	40%
\$20,000 to \$55,000	3%	\$55,000,000	26%

compensated for or canceled out by an adjustment that attempts to take into account the ownership of corporate wealth.

In 1828 corporations, mainly banks and insurance companies, were assessed for \$23,984,660 or twenty-one per cent of the city's total estate of \$112,019,533 (exclusive of partnerships). It is probably impossible to track down the owners of all corporate wealth; records, inadequate to begin with, have been lost. Yet for all the imprecision attending the attribution of corporate capital, certain conclusions can be drawn that affect significantly the distribution of wealth.

Poor men and for that matter the great bulk of the city's population owned either nothing or merely minuscule portions of such capital. The minimum cost of a share, typically fifty dollars to one hundred dollars, priced out such people. As Cadwallader C. Colden and Peter A. Jay pointed out in behalf of the Bank for Savings in 1823, "a depositor is not a stockholder."⁶⁶ The directors of the corporations regularly listed in the annual New York City directories were overwhelmingly the merchant elite, many of the same individuals forming a kind of interlocking directorate over the great city's banks and insurance companies.⁶⁷ These directors were required to own stock in their corporations.⁶⁸ According to an insider, himself an

⁶⁶ Quoted in Charles E. Knowles, *History of the Bank for Savings in the City of New York, 1819-1929* (New York, 1929), 70-71.

⁶⁷ Appearing over and over again as directors were the names of New York City's mighty: William Bayard; Henry Rutgers; Archibald Gracie; Richard Varick; Duncan P. Campbell; Gilbert and William H. Aspinwall; Peter A. Jay; George Arcularius; Henry Eckford; Philip Hone; Jeremiah Thompson; Henry Remsen; Chancellor James Kent; William Few; I. P. Phoenix; S. B. Ruggles; James and Robert Lenox; Daniel Embury; James De Peyster; John Jacob and William B. Astor; James H. Suydam; J. W. Hamersley; Preserved Fish; Gerard, Henry, and John C. Beekman; Thomas Addis Emmet; Leonard Blecker; David Hosack; Isaac Kip; John Mason; Peter Schermerhorn; Peter W. Livingston; Nathaniel Prime; Ogden Hoffman; Edward R. and John Q. Jones; Peter Goelet; David S. Kennedy; Aquila G. Stout; Isaac and John Heyer; Arthur Tappan; Mathew Clarkson; Frederick Schuchardt; C. V. S. Roosevelt; John D. Wolfe; A. T. Stewart; Jacob and George Lorillard; Peter Lorillard, Jr.; Al[is]on Post; John Rankin; Daniel Lord; Jacob LeRoy; Nicholas Dean; John Sampson; Thomas T. Woodruff; Myndert Van Schaick; Ambrose C. Kingsland; David H. Haight; Alonzo A. Alvord; Stephen A. Halsey; William B. Crosby; Caleb O. Halsted; James Brown; Henry Brevoort; Robert C. Cornell; Rufus L. Lord; George Bruce; Gideon Tucker; John T. Irving; and other members of the great city's elite of wealth.

⁶⁸ When Preserved Fish testified in the case of *City Fire Insurance Company v. Elisha Bloomer* in 1834, in response to the question as to when he resigned as director, Fish answered: "I resigned six months ago. The fact is, rather, that I sold out my stock, which precluded my being a Director after that time." New York City Court for the Correction of Errors, *The City Fire Insurance Company of the City of New-York, Respondents, Elisha Bloomer, Impleader with others [including Richard K. Haight and David H. Haight]* (New York, 1841), 19-20.

officer in several New York City banks during the era, directors usually "own[ed] in the aggregate a considerable portion of the stock" in their companies. They had been chosen in the first place "for their wealth, commercial experience, and influence in attracting to the institution a good class of dealers."⁶⁹ Precise information available for a number of contemporary banks and insurance companies discloses that a small number of directors owned almost all the capital in their corporations.⁷⁰ When allowance is made for the inordinate share of corporate wealth owned by the elite it is likely that in New York City the richest one per cent actually owned as much as forty-one per cent and the next wealthiest three per cent owned twenty-two per cent of all wealth in 1828. By 1845 the wealthiest one per cent would have owned one-half of all wealth, while the upper four per cent owned eighty-one per cent of the city's total wealth.⁷¹

The evidence for Brooklyn is unusually interesting because it permits a comparison of the degree of equality that obtained in the village of 1810, populated by fewer than 5,000 persons, with the bustling city of 1841, whose population of about 41,000 placed Brooklyn seventh among the nation's cities. The richest wealth holders of the early nineteenth-century village held a slightly larger portion of Brooklyn's wealth than had typically been controlled by the upper tenth in the urban Northeast late in the "Revolutionary era."

Although the poorer half of the population owned only a tiny fraction of Brooklyn's wealth in 1810, the fact that seven out of eight families paid taxes on some property, even if slight, suggests that few residents of the community could be classified as propertyless proletarians. By 1841 important changes had occurred.

The distribution of wealth in the commercial Brooklyn of 1841 was strikingly similar to the division that obtained in its great neighbor across the East River. By 1841 the poorest two-thirds of Brooklyn's population

⁶⁹ James Sloane Gibbons, *The Banks of New York* (New York, 1858), 21. See also Winslow, *Biographies of Successful Philadelphia Merchants*, 197, for reference to the large quantity of bank and insurance stock owned by wealthy individuals.

⁷⁰ The fifteen wealthiest subscribers, all of them in the city's mercantile upper crust, owned more than half the shares of the rechartered Chemical Bank in 1844. *History of the Chemical Bank* (New York, 1913), 34. According to Samuel B. Ruggles' biographer, the \$10,000 worth of shares in the Bank of Commerce owned by Ruggles in 1840 was the minimum that a director could own; each of the bank's seventeen directors usually owned more than that amount. D. G. Brinton Thompson, *Ruggles of New York: A Life of Samuel B. Ruggles* (New York, 1946), 39-40. See also *Records of Guaranty Trust Company of New York* (New York, n.d.); Philip G. Hudnut, *The Merchants' National Bank of the City of New York* (New York, 1903), 4; Henry W. Domett, *A History of the Bank of New York 1784-1884* (New York, 1884), much fuller and more valuable than Allan Nevins, *History of the Bank of New York and Trust Company 1784 to 1934* (New York, 1934); Wilson, *Memorial History of the City of New York, Biographical Volume*, 176-77; MS diary of Philip Hone, 19: 83; *Charter of the Seventh Ward Fire Insurance Company of New-York* (New York, 1839); Lossing, *History of New York City*, 2: 487.

⁷¹ These figures are based on a formula that attributes half of corporate capital to outsiders (which is probably overgenerous) and the rest to elite taxpayers. The worthwhileness of this rough rule was confirmed by an authority on both statistics and finance, Samuel Richmond, author of *Statistical Analysis* (New York, 1968).

TABLE 5. Distribution of Wealth in Brooklyn in 1810

<i>Level of Wealth</i>	<i>Percentage of Population</i>	<i>Approximate Non-corporate Wealth Owned</i>	<i>Percentage Non-corporate Wealth</i>
\$15,000 or more	1%	\$262,400	22%
\$ 4,000 to \$15,000	7%	\$383,122	33%
\$ 2,500 to \$ 4,000	6%	\$137,944	11%
\$ 1,000 to \$ 2,500	20%	\$290,000	25%
\$ 500 to \$ 1,000	12%	\$ 67,500	6%
Under \$500	54%	\$ 30,000	3%

owned less than one per cent of its wealth, with only about one out of five families (exclusive of nonresident taxpayers) taxed on any property at all. Corporate wealth had come to be a factor of some significance, accounting for seven per cent of the total. As in New York City, this type of wealth was evidently monopolized by the elite. Data on the holdings and ownership of the Fulton Ferry, the Brooklyn White Lead Company, the Long Island Bank, and the Brooklyn Fire Insurance Company—firms assessed for about seventy-five per cent of the city's corporate wealth—indicate that the percentage of assessed wealth owned by the richest one per cent was closer to forty-five than forty-two per cent.⁷² In addition, Brooklyn's wealthiest taxpayers for the most part admitted to no personal wealth whatever. If the personal wealth of the richest one per cent is treated as though it equaled in value their real property, and when the under-assessment of the latter form of property is accounted for by working into our estimate an adjustment that presumes ownership of three-quarters of corporate capital by Brooklyn's elite (outsiders owned close to one-quarter), Brooklyn's richest one per cent emerge with one-half of their city's wealth.

The trend toward increasingly unequal distribution of wealth in the

TABLE 6. Distribution of Wealth in Brooklyn in 1841

<i>Level of Wealth</i>	<i>Percentage of Population</i>	<i>Approximate Non-corporate Wealth Owned</i>	<i>Percentage Non-corporate Wealth</i>
\$50,000 or more	1%	\$10,087,000	42%
\$15,000 to \$50,000	2%	\$ 4,000,000	17%
\$ 4,500 to \$15,000	9%	\$ 5,730,000	24%
\$ 1,000 to \$ 4,500	15%	\$ 2,804,000	12%
\$ 100 to \$ 1,000	7%	\$ 1,000,000	4%
Under \$100	66%		

⁷² The Brooklyn directories for the period list valuable data on boards of directors. See also Henry R. Stiles, *The Civil, Political, Professional and Ecclesiastical History and Commercial and Industrial Record of the County of Kings and the City of Brooklyn New York from 1683 to 1884* (New York, 1884), 1: 143, 154, 2: 213, 436, 620-23; Stiles, *A History of the City of Brooklyn* (Brooklyn, 1867-70), 3: 543; "A Director," *A Historical Sketch of the Fulton Ferry* (Brooklyn, 1839), app., 8-11; *History and Commerce of Brooklyn* (New York, 1893); and *Charter and By-Laws and Regulations of the Brooklyn Savings' Bank* (Brooklyn, 1836).

antebellum era was not confined to the great cities of the Northeast. While the pattern of distribution in rural communities and small towns was not as skewed as it was in large urban centers, inequality in the former milieus was dramatic and worsening. In Hamilton, Ontario, "a small commercial lakeport almost entirely lacking in factory industry, with a population just over 14,000" shortly after mid-century, the poorest four-fifths of the population owned less than four per cent of the town's property, in contrast to the richest tenth, who owned almost ninety per cent.⁷³ As small Massachusetts communities, such as Worcester, became increasingly urbanized, the rich became relatively richer, the numbers of propertyless citizens increased drastically, and "patterns of ownership" became "sharply skewed."⁷⁴

Recent research indicates that on the eve of the Civil War the pattern of maldistribution in Philadelphia and in a number of Southern and Western cities was quite similar to the inequality that prevailed in New York City, Brooklyn, and Boston in the 1840s. By 1860 the wealthiest one per cent of Philadelphia's population evidently owned one-half, while the lower eighty per cent held only three per cent of the city's wealth. In Baltimore, New Orleans, and St. Louis the richest one per cent of the population owned about two-fifths, the richest five per cent better than two-thirds, and the upper ten per cent more than four-fifths of the wealth. An impressionistic recent account of Galveston at mid-century finds that the affluent social and economic elite were one hundred times wealthier than their fellow citizens, the wealth of the former group contrasting "strikingly with that of their nearest neighbors." The division of property was not as unequal in rural counties, Southern or Western, although even in such areas the distribution has been found to have been skewed to a surprising extent. In cotton counties the wealthiest five per cent of landholders held more than two-fifths of the wealth, while the upper ten per cent owned almost three-fifths. (According to Gavin Wright, a close student of rural wealth distribution, the actual degree of inequality was greater than the census data indicate.) While wealth was more equally distributed on the northeastern frontier, even there the upper tenth by 1860 held close to two-fifths of taxable wealth. In the words of two modern students, property holding on the Michigan frontier became "more concentrated" with the passage of time, while the distribution of wealth "scarcely supports the typical American image of the frontier as the land of promise for the poor, ambitious young man."⁷⁵

⁷³ Katz, "Patterns of Inequality."

⁷⁴ Doherty, "Property Distribution in Jacksonian America," introduction, 2, 4. Doherty found that only towns that languished or stagnated resisted the trend toward greater inequality.

⁷⁵ George Blackburn and Sherman L. Richards, Jr., "A Demographic History of the West: Manistee County, Michigan, 1860," *Journal of American History*, 57 (1970): 618, 613; Gavin Wright, "'Economic Democracy' and the Concentration of Agricultural Wealth in the Cotton South, 1850-1860," *Agricultural History*, 44 (1970): 63-94; Blumin, "Mobility in a Nine-

During the age of egalitarianism wealth became more unequally distributed with each passing season. Shared less equally, even at the era's beginnings, than it had been a generation or two earlier, in the aftermath of the Revolution, wealth became concentrated in the hands of an ever smaller percentage of the population. The trend persisted through the 1850s, resulting in wider disparities than ever by the time of the Civil War.⁷⁶ Far from being an age of equality, the antebellum decades were featured by an inequality that surpasses anything experienced by the United States in the twentieth century.⁷⁷

ACCORDING TO GERHARD LENSKI, the central question in studying social stratification is: "Who gets what and why?"⁷⁸ For the era of Tocqueville the answer to the first part of this question is clear enough. The few at the top got a share of society's material things that was disproportionate at the start and became more so at the era's end. Why they did is of course more difficult to explain.

It may be, as Lenski has argued, that in a free market system "small inequalities tend to generate greater inequalities and great inequalities still greater ones."⁷⁹ Even if Lenski's comment is true, it is more descriptive than analytical, while leaving unanswered the question: Why? The explanation, popular since Karl Marx's time, that it was industrialization that pauperized the masses, in the process transforming a relatively egalitarian

teenth-Century American City," 46-48; Gallman, "Trends in the Size Distribution of Wealth in the Nineteenth Century," 1-25; and Wheeler, *Beginnings of Urban Growth in Texas, 1836-1865*, 131.

⁷⁶ In New York City by 1863 roughly sixty-one per cent of all income was made by the 1,600 families that constituted the upper one per cent of income earners: computed from the figures in *The Income Record, A List Giving the Taxable Income for the year 1863, of the Residents of New York [City]* (New York, 1865). It is likely that wealth was more badly distributed, since inheritance accounts for so much of it, in contrast to the democratic rule governing income that in a sense all people start from scratch no matter how disparate their earnings. Rufus S. Tucker long ago noted that the 1863 tax record for New York City showed "less concentration and less inequality than actually existed." See Tucker, "The Distribution of Income Among Income Taxpayers in the United States, 1863-1935," *Quarterly Journal of Economics*, 52 (1938): 561-62.

⁷⁷ Modern scholars differ in interpreting the data on income distribution. Yet even Michael Harrington and Gabriel Kolko, whose estimates reveal the greatest amount of inequality, attribute percentages of income to the upper brackets that are far smaller than the upper one per cent of New York City controlled in income in 1863 or in wealth in 1845. See Kolko, "Economic Mobility and Social Stratification," *American Journal of Sociology*, 63 (1957): 38; Kolko, *Wealth and Power in America: An Analysis of Social Class and Income Distribution* (New York, 1962); and Michael Harrington, *The Other America: Poverty in the United States* (New York, 1962). See also Tucker, "The Distribution of Income Among Income Taxpayers in the United States," 569, 585; Herman P. Miller, *Income of the American People* (New York, 1955); United States Bureau of the Census, *How Our Income is Divided* (Washington, 1963); Robert J. Lampman, *Changes in the Share of Wealth Held by Top Wealth-Holders, 1922-1953* (New York, 1960); and Lampman, *The Share of Top Wealth-Holders in National Wealth, 1922-1956* (Princeton, 1962). For a comparison with Britain, see Roy Perrot, *The Aristocrats, A Portrait of Britain's Nobility and Their Way of Life Today* (New York, 1968), 76-77.

⁷⁸ Gerhard Lenski, *Power and Privilege: A Theory of Social Stratification* (New York, 1966), 3.

⁷⁹ *Ibid.*, 341.

social order, appears wanting. Vast disparities between urban rich and poor antedated industrialism. Commercial wealth, as surely as industrial, enabled its fortunate inheritors to command a disproportionate share of society's good things and the children of the fortunate to hold a still greater share. A massive internal migration, above all of younger, marginal persons of little standing, into and out of the nation's cities increased both the power and the share of wealth commanded by more substantial and therefore more stable elements.⁸⁰ It is hard to disagree with Robert Gallman's generalization that "there were forces at work in the American economy during the nineteenth century that tended to produce greater inequality in the distribution of wealth over time."⁸¹ A not insignificant task of future scholarship will be to ascertain as precisely as possible the nature of these "forces." I would venture the judgment that the transportation revolution and the *de facto* single national market it helped create made possible and indeed decisively fostered great increases in profit-making opportunities even before the victory of industrialism, while the system of inheritance and the minimal influence of the non-property-owning classes enabled private accumulators to command a larger share of society's product than they would be able to in a later era of vastly greater absolute productivity and profits. Amid all the hulabaloo about the "common man" during the era, he in fact got what was left over.

It has long been argued that equality of opportunity if not of condition prevailed in antebellum America, the era's numerous success stories testifying to the rule of the former principle. In David Potter's language, in America equality did not mean the possession of uniform wealth so much as "parity in competition."⁸² The evidence, however, indicates that if dramatic upward climbs were more fanciful than real in Jacksonian America, competition was also marked by anything but parity. The absence of legal disabilities did not mean that poor men started the race for success on equal terms with their more favored contemporaries.

According to Charles Astor Bristed, the young man who hoped to gain entry into New York's upper one thousand was one who, possessed of "fair natural abilities, adds to these the advantages of inherited wealth, a liberal education and foreign travel."⁸³ It need hardly be pointed out that the travel and liberal education mentioned by Bristed were not available to most Americans.⁸⁴ Rather, they were accessible to men such as Abram C.

⁸⁰ See Doherty, "Property Distribution in Jacksonian America," 4-5; Stephan Thernstrom and Peter R. Knights, "Men in Motion: Some Data and Speculations about Urban Population Mobility in Nineteenth-Century America," *Journal of Interdisciplinary History*, 1 (1970): 29-30; Michael Katz, "Patterns of Inequality," 5; and Stuart Blumin, "The Restless Citizen: Vertical Mobility, Migration and Social Participation in Mid-Nineteenth Century America," a 1970 unpublished version of a paper (on Kingston, New York) presented at the Conference on Social Science Concepts in American Political History, Oct. 24, 1969, at Brockport, New York.

⁸¹ Gallman, "Trends in the Size Distribution of Wealth," 11.

⁸² Potter, *People of Plenty*, 91-92.

⁸³ Bristed, *The Upper Ten Thousand*, 9.

⁸⁴ In Boston, where the importance of education was hardly understressed, college attendance

Dayton, son of "an opulent merchant" of New York City, who had "all the accomplishments that education, travel and wealth could give." They were available to Andrew Gordon Hamersley, who inherited from his father a fortune, which, by "judicious management," he succeeded in substantially enlarging. Like other of his golden contemporaries he never went into business, owing his success rather to his name, his original possessions, and his "entertaining conversation and courtly manner." They were available to John Collins Warren, Valentine Mott, David Hosack, and Philip Syng Physick, brilliant physicians all, who from childhood had moved in the most rarified circles, attending the greatest universities and studying with the most learned masters at home and abroad, accumulating much wealth largely because they had much to begin with. Means rather than need gave one access to the services of these eminences.⁸⁵

It is of course possible that innate ability or a fortunate genetic inheritance accounted for the success achieved by most of the era's socio-economic elite. Such traits no doubt played a significant part in some cases. The biographical data indicate, however, that a material inheritance was the great initial advantage that enabled most of those fortunate enough to have it to become worldly successes. Robert A. Dahl has contended that the era was marked by a "cumulative inequality: when one individual was much better off than another in one resource, such as wealth, he was usually better off in almost every other resource," including political influence.⁸⁶ It is clear that almost all of the era's successful and wealthy urbanites had initially been much better off than their fellows in possessing the "resource" of wealth.

The race was indeed to the swift, but unfortunately the requisite swiftness was beyond the power of ordinary men to attain. For this swiftness was of a special sort. Unlike the speed of thoroughbred horses, which is a rare but a natural if inbred gift, the ability to cover great ground in the race for human material success appeared to depend less on the possession of innate abilities than on the inheritance of the artificial gifts of wealth and standing. During the "age of the common man" opportunity was hardly more equal than was material condition.

The evidence presented here has been drawn primarily from four large Northeastern cities, communities that were hardly typical of the nation as a whole. Yet, as has been indicated, earlier detailed if not quantitative studies of antebellum Natchez, Detroit, Cincinnati, and other

during the era was confined to the few. In 1829 one person in 620 attended; in 1833, one in 929; in 1837, one in 748; in 1841, one in 873; and in 1845, one in 1,012. Shattuck, *Census of Boston for the Year 1845*, 74. See also Weeks, *Prominent Families of New York*, 524; and Daniel Scott Smith, "Cyclical, Secular, and Structural Changes in American Elite Composition," *Perspectives in American History*, 4 (1970): 369.

⁸⁵ When Philip Hone's brother John was seriously ill in 1832 he was attended by Hosack, Mott, and the eminent Dr. Hugh McLean. MS diary of Philip Hone, 4: 121.

⁸⁶ Dahl, *Who Governs?* 85.

Southern and Western cities revealed patterns of increasing inequality and social rigidity along the "urban frontier."⁸⁷ Recent quantitative studies have disclosed that wealth was distributed most unequally in agricultural areas, in small towns, and in Baltimore, New Orleans, and St. Louis, even if the precise patterns of maldistribution were not quite as skewed as for the great cities of the Northeast. The data on the origins, the immensity, the durability, and the distribution of wealth in the United States during the second quarter of the nineteenth century therefore suggest that egalitarianism, in accord with Webster's definition of a myth, may have existed more in the imagination of men than in the lives they led.

The limitation of such evidence lies precisely in its inability to penetrate the imagination or the thinking and feeling of men, in Freudian terms, their conscious and unconscious. A significant component of what I have called the "egalitarian myth," in being immaterial or metaphysical, is resistant to the quantitative method. There appear to be important questions that quantitative studies have not answered and may be unable to answer.⁸⁸ Who would aspire to a comprehensive grasp of the "age of egalitarianism" must consider such questions. What has been attempted in this article has been the measurement of the measurable—or, to be more precise, the measurement of some of the measurable. The behavior and influence of the rich, phenomena that are measurable if difficult to gauge, remain to be evaluated.⁸⁹ It may be, as one scholar has recently written, that the latter kind of information is "more crucial for history than the social origins" of the elite.⁹⁰ The purpose of this investigation has not been the grandiose one of answering the most crucial questions—whatever they may be—about Jacksonian society. I have chosen, rather, to discuss important questions, the answers to which may be crucial to an understanding of that society. The evidence on the backgrounds and the wealth of the rich indicates that the second quarter of the nineteenth century was something other than an age of egalitarianism. Since ancient historical rubrics confirmed by long usage are powerfully resistant to scholarly attempts to discard them, historians might spend their time more fruitfully by rethinking their estimates of the period in the light of the new evi-

⁸⁷ Wade, *The Urban Frontier*; James, *Antebellum Natchez*; Wheeler, *Beginnings of Urban Growth in Texas*; McCoy, "Political Affiliations of American Economic Elites"; Aaron, "Cincinnati, 1818–1838."

⁸⁸ Edward Pessen, "Should Labor Have Supported Jackson? or Questions the Quantitative Studies Do Not Answer," paper read at the annual meeting of the Organization of American Historians, Apr. 18, 1969, in Philadelphia; published in revised form as "Jacksonian Quantification: On Asking the Right Questions," in Herbert Bass, ed., *The State of American History* (Chicago, 1970), 362–72.

⁸⁹ In a subsequent study I hope to evaluate the residential and marital patterns, the private lives, the social and cultural role, and the political influence of the wealthy.

⁹⁰ Daniel Scott Smith, "Cyclical, Secular, and Structural Changes in American Elite Composition," 372. A similar statement is made by Michael H. Frisch, "The Community Elite and the Emergence of Urban Politics: Springfield, Massachusetts, 1840–1880," in Thernstrom and Sennett, *Nineteenth-Century Cities*, 277.

dence than by trying to replace old labels with new. Truer captions will follow on the heels of truer explanations of the nature of the era.

APPENDIX:

A Note on Sources of Information on the Families of the Rich

SINCE SEVERAL SOURCES were consulted for each of two thousand individuals, and valuable material, though sometimes in snippets, was obtained from many of the nongenealogical writings, any attempt even to list all of these sources would run into dozens of pages. I sought clues in many hundreds of manuscript and printed sources for each of the cities studied and was assisted by cooperative librarians who simply turned me loose in their stacks, enabling me to examine every published item on the period.

Unless their data were confirmed by reliable contemporaries, little stock was placed on the evidence of the famous biographical encyclopedia, primarily for its thinness but also for its unreliability. See Allan Nevins, *The Gateway to History* (Boston, 1938) (especially p. 202 of the reprint of a portion of Nevins' essay, in Robin W. Winks, ed., *The Historian as Detective: Essays in Evidence* [New York, 1969]), for a discussion of the actual invention of material in Appleton's *Cyclopaedia of American Biography*; and Nicholas B. Wainwright, ed., *A Philadelphia Perspective: The Diary of Sidney George Fisher Covering the Years 1834-1871* (Philadelphia, 1967), iii, on an inaccuracy in the *Dictionary of American Biography*. The problem with the latter estimable source for our purposes is that so many of its contributors had little interest in the parental status of their subjects. See Daniel Scott Smith, "Cyclical, Secular, and Structural Change in American Elite Composition," *Perspectives in American History*, 4 (1970): 370, for a brief discussion of some of the inadequacies of the *DAB* material. I have discounted entirely Moses Beach's *Wealthy Citizens* pamphlets, since Beach's cavalier methods in publishing these listings disqualify them as reliable sources. I also agree with the editor of the authoritative *New York Genealogical and Biographical Record*, who said of Joseph Scoville's *Old Merchants of New York* that "the character of this entertaining, gossipy work is not such as to entitle it to any weight," for all its author's ability and experience. *NYGBR*, 3 (1872): 180. Specific Scoville errors are also pointed out in *ibid.*, 61 (1930): 342-43. Genealogical data are hardly foolproof, even when drawn from such reliable sources as the *NYGBR*, the *New England Historical and Genealogical Register* (hereafter *NEHGR*), the *Publications of the Genealogical Society of Pennsylvania*, and the *Pennsylvania Magazine of History and*

Biography (the journal of the Historical Society of Pennsylvania) and even when every item in every issue of these sources, commencing with the first issue of the *NEHGR* in January 1847, was scrutinized for useful leads. Family genealogies were also examined. If, after consulting an early volume of the former sources, the researcher is confident that he has secured accurate data, he would be disappointed to discover that subsequent volumes sometimes modify drastically previous biographical statements. As for the latter sources, the tendency of some genealogists to attribute a too exalted status to families of their subjects is counterbalanced somewhat by the disposition to attribute humble origins to substantial men. For comment on the latter tendency, see Benjamin D. Silliman, "Personal Reminiscences of Sixty Years at the New York Bar," 1: 226-43; and John F. Watson, *Annals of Philadelphia in the Olden Time* (Philadelphia, 1842), 1: 530. For a pointed criticism of the tendency of too many genealogists to prefer biographical to social data, see Edward P. Cheyney, "Thomas Cheyney, A Chester County Squire: His Lesson for Genealogists," *Pennsylvania Magazine of History and Biography*, 60 (1936): 209, 221-22. Other informative criticisms are Roy F. Nichols, "The Genealogist and the Historian," *Publications of the Genealogical Society of Pennsylvania*, 14 (1942): 1-2; and Z. S. Fink, "Some Genealogical Absurdities," *NYGBR*, 52 (1921): 295-96. A useful essay, written from a different point of view, is John F. Lewis, "Some Genealogical Obstacles Considered," *Publications of the Genealogical Society of Pennsylvania*, 3 (1906): 81-104. Among the sources consulted at the historical societies and in other libraries were the private papers of such families as the Stuyvesants, Hendrickses, Fishes, Hones, Grosvenors, Beekmans, Lorillards, Suydams, Schermerhorns, Tillotsons, Stronges, Van Rensselaers, Brinckerhoffs, Astors, Hallocks, Noahs, Kings, Bennetts, Livingstons, Griswolds, Brevoorts, Dodges, Allens, Phelps, Enos, Whitneys, Aspinwalls, Emmets, Bories, Fishers, Careys, Brimmers, Appletons, Searses, Lawrences, Brookses, Everetts, and Furmans. While such sources are invaluable for many purposes, they yield less fruitful data on the status of families, in some cases, than do some of the excellent local histories written in the nineteenth century by competent men who often knew personally the elite families they described. John Latting, early editor of the *NYGBR*, had studied law under Francis B. Cutting and worked with Caleb S. Woodhull. Latting's perfectionism in authenticating the most minute items, as he rummaged through probate records, published and unpublished documents, or sought out survivors, is assurance of his reliability. For examples of the comforting finickiness of this man, who was widely respected and sought after by contemporary historians and genealogists, see the correspondence between Latting and the historian of Long Island, Henry Onderdonk, Jr., in the Onderdonk Papers at the Long Island Historical Society. That Henry Simpson, mid-nineteenth-century biographer of Philadelphia's elite, was aided in his re-

search by such eminences as Horace Binney, Samuel Breck, Henry W. Gilpin, Charles J. Biddle, and Thomas Balch, adds to the credibility of his *Lives*. John F. Watson's personal encounters with some of the elite whose family histories he sketched in 1842 similarly induce respect for his portrayals; see J. Thomas Scharf and Thompson Westcott, *History of Philadelphia, 1609-1884* (Philadelphia, 1884), 2: 1169. Certainly the value of Abraham Ritter's charming and anecdotal account of *Philadelphia and Her Merchants* (Philadelphia, 1860) is enhanced by the evidence that dozens of the early nineteenth-century merchants he discusses respected his judgments. Of course the character of a genealogist is a surer clue to his reliability than the fact that he was contemporary with his subject. And even sensible biographers had foibles. Thompson Westcott was an indefatigable researcher and invaluable source for all things Philadelphian in the mid-nineteenth century. Yet I have discovered that some of his biographical vignettes either ignored or omitted pertinent data and in a few cases simply repeated unverified versions written by predecessors. See Westcott, comp., *Biographies of Philadelphians* (Philadelphia, 1861), and Westcott's four-volume "Historical Scrap Book concerning the City of Philadelphia," collected 1848-52, which is at the Historical Society of Pennsylvania. Unusually helpful were Horace Lyman Weeks, *Prominent Families of New York* (New York, 1897); Stephen Winslow, *Biographies of Successful Philadelphia Merchants*; Mary Louise Booth, *History of the City of New York* (New York, 1859); Charles Andrew Ditmas, *Historic Homesteads of Kings County* (New York, 1909); Freeman Hunt, *Lives of American Merchants* (New York, 1857), and Hunt's *Merchants' Magazine*; Martha Lamb and Mrs. Burton Harrison, *History of the City of New York* (New York, 1877-96); Frank Willing Leach, *Old Philadelphia Families* (Philadelphia, 1907-13); Charles Morris, *Makers of Philadelphia* (Philadelphia, 1894); John William Leonard, *History of the City of New York 1609-1909* (New York, 1910); James J. Levick, *The Early Physicians of Philadelphia* (Philadelphia, 1886); Benson J. Lossing, *History of New York City* (New York, 1884); Emil Paxson Oberholtzer, *Philadelphia, A History* (Philadelphia, n.d.); Oberholtzer, *Literary History of Philadelphia* (Philadelphia, 1906); I. N. Phelps Stokes, *Iconography of Manhattan Island* (New York, 1916-28); Josiah Quincy, *Figures of the Past* (Boston, 1883); Edwin A. Stone, *A Century of Boston Banking* (Boston, 1894); John F. Trow, ed., *Boston Past and Present* (Boston, n.d.); George H. Blelock, *Boston Past and Present* (Boston, 1874); John Langdon Sibley, *Biographical Sketches of Graduates of Harvard University* (Cambridge, Mass., 1881); Clifford K. Shipton, *Biographical Sketches of Those Who Attended Harvard College in the Classes 1690-1700* (Cambridge, Mass., 1933); William R. Cutter, *Genealogical and Personal Memoirs Relating to Families of Boston and Eastern Massachusetts* (New York, 1908); and Mary Caroline Crawford, *Famous Families of Massachusetts* (Boston, 1930).

For testimony on the reliability of Crawford, Lamb, and Weeks, see the *New York Genealogical and Biographical Record*, 61 (1930): 219; 24 (1893): 92; 29 (1898): 182. Modern scholarly biographies such as Philip L. White's *The Beekmans of New York in Politics and Commerce*; James A. Rawley, *Edwin D. Morgan, 1811-1883, Merchant in Politics* (New York, 1955); D. G. Brinton Thompson, *Ruggles of New York: A Life of Samuel B. Ruggles* (New York, 1946); Richard Lowitt, *A Merchant Prince of the Nineteenth Century: William E. Dodge* (New York, 1954); Bertram Wyatt-Brown, *Lewis Tappan and the Evangelical War Against Slavery* (Cleveland, 1969); and Elva Tooker, *Nathan Trotter, Philadelphia Merchant, 1787-1853* (Cambridge, 1955)—to name only a few—are first rate on particular families. The twenty-eight folio volumes containing Philip Hone's diary, like the manuscript diaries of George Templeton Strong (for the brave soul who can manage Strong's handwriting) and Edward Neufville Tailer, all at the New-York Historical Society, the diary of Sidney George Fisher at the Historical Society of Pennsylvania, or the journals of Gabriel Furman and John Baxter in the Long Island Historical Society are filled with intimate glimpses into the backgrounds of the urban elite by men who knew them because they were of them. James G. Wilson's *Memorial History of the City of New York* including an "anonymously" edited fifth biographical volume; Justin Winsor's *Memorial History of Boston* (Boston, 1884); and John Russell Young's *Memorial History of the City of Philadelphia* (New York, 1895), are examples of the "memorial histories," histories of bench and bar, the medical, religious, and business histories, and many dozens of local accounts that offer invaluable data on the elite of the great cities. For Brooklyn nothing compares in value with Henry R. Stiles' monumental *Civil, Political, Professional and Ecclesiastical History and Commercial and Industrial Record of the County of Kings and the City of Brooklyn New York from 1683 to 1884* (New York, 1884), a sprawling, detailed account filled with family vignettes written by an insider who was an indefatigable genealogist. Stiles was a founder and the first librarian of the Long Island Historical Society as well as the first president of the New York Genealogical and Biographical Society. Indispensable for Brooklyn, too, are the 160 scrapbooks at the Long Island Historical Society, a treasurehouse filled with much trivia side by side with invaluable manuscript and other data on the Cortelyou, Hoyt, Gerritson, Couwenhoven, Martense, Rapelye, Schermerhorn, Remsen, Hicks, Willoughby, and other elite families.

"A Necessary Cruelty": The Emergence of Official Anti-Semitism in Poland, 1936–39

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THE DEATH OF Marshal Józef Piłsudski in May 1935 presented his successors with an urgent need to solve the twin problems of the internal disintegration of their own camp, the so-called *Sanacja*, and of the serious lack of any viable basis for their continued rule among the general community. The strategy finally selected to achieve these related objectives was predicated upon gaining the support of one segment of Polish society at the expense of alienating the remainder of the population, Poles as well as national minorities, from the regime that governed them. To the dismay of the vast majority of both native and foreign observers, the open espousal of anti-Semitism as a social, cultural, economic, and political policy sanctioned by the Polish state emerged as one of the chief elements of this strategy, thereby reversing the political practice followed by Piłsudski during his nine-year period of control over the government and, in the process, contributing to the internal and external weakening of the Polish Republic at a time when it required the good will and united front of its citizens and allies alike.

Throughout Piłsudski's lifetime the most determined challenges to his programs and policies had come from the National Democratic movement of Roman Dmowski, popularly known as the *Endecja*. In the years following the Marshal's seizure of power in May 1926, however, it had gradually become apparent that the hostility between the two camps was largely a matter of personal differences. In terms of ideology and outlook the movements possessed far more common ground than appeared possible in the polemical exchanges of the two leaders.¹ The successors of Piłsudski there-

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¹ This theme has been stated often, but its best presentation is in Andrzej Micewski's two recent studies, *Z geografii politycznej II Rzeczypospolitej* (Warsaw, 1966), and *W cieniu Marszałka Piłsudskiego* (Warsaw, 1968). Polish sociologist Józef Chałasiński discusses the social origins and outlooks of both camps in his *Przeszłość i przyszłość inteligencji polskiej* (Rome, 1947), 140–59. See also the two articles by Alexander Hertz, "The Case of an East European Intel-

fore decided to direct their efforts primarily at the Polish nationalists, particularly Dmowski's National party (SN) and both the ABC and Falanga factions of the more extreme National-Radical Camp (ONR).² Like the majority of the Sanacja the nationalists believed firmly in a powerful, centralized government, asserting the supremacy of the Polish nation and the Catholic Church over the national and religious minorities that comprised nearly one-third of the country's population; unlike most of Piłsudski's associates, however, the nationalists were dedicated anti-Semites. Dmowski, whose long and intimate involvement in Russian political circles had left an indelible stamp upon his political thinking, made anti-Semitism a key tenet of his ideology and, hence, a cornerstone of his party's policy. As early as 1914 he had declared that no Jew could ever be a true conservative in European society, for "the whole tradition of that society is alien to him, is opposed to everything with which the Jewish soul has become imbued in the course of immutable generations. The Jew treats with aversion the entire past of European nations; he harbors hatred toward their religions and looks upon all the hierarchies that have arisen in these societies as he does upon the usurpers who have taken the place due his 'chosen people.'" For these reasons, concluded Dmowski, the Jew was the most dangerous enemy of Polish civilization, bent upon destroying all vestiges of those institutions and ideals that a Pole would hold dear.³ Since the success of the regime's strategy hinged in large part upon the reconciliation of these two historic trends of Polish political thought—the socialism of Piłsudski, emphasizing the state, and the National Democracy of Dmowski, with its stress upon the Polish nation—it is hardly surprising that anti-Semitism was envisioned as the main bridge spanning the chasm between the two rivals. Unfortunately the demographic, economic, and social structure of the Jewish population made it an excellent target for the frustrations nurtured by many Poles. In 1937 Poland's three and a half million Jews comprised approximately ten per cent of the total population and played an economic and cultural role far disproportionate to their numbers in rural as well as urban areas.⁴ The tendency of most Jews to resist as-

ligencja," *Journal of Central European Affairs*, 11 (1951): 10-26, and "The Social Background of the Pre-War Polish Political Structure," *ibid.*, 2 (1942): 145-61.

² In 1935 several young Endeks, notably Zdzisław Stahl, Ryszard Piestrzyński, and Klaudiusz Hrabek, had broken with the SN and joined with the Sanacja. This trend was urged upon the other Endeks by Władysław Grabski, former premier and finance minister and himself a moderate Endek, in his book *Idea Polski* (Warsaw, 1935), as well as in the ONR organ *Ruch Młodych*, Jan. 1936, p. 1. "Piłsudczycy narodowi, czyli otwarcie na prawo," chapter 6 of Micewski's *W cieniu* (pp. 200-47), and three publications by Zdzisław Stahl give an insight into the motives and actions of these Nationalists. Stahl, *System Dmowskiego wczoraj i dziś* (London, 1953), *Polityka polska po śmierci Piłsudskiego* (Warsaw, 1936), and *Idea i walka* (Warsaw, 1938).

³ *Upadek myśli konserwatywnej w Polsce* (Warsaw, 1914), 136.

⁴ There is surprisingly little reliable material on this vital topic of Polish history. Szyja Bronstejn's *Ludność żydowska w Polsce w okresie międzywojennym* (Warsaw, 1963) is the best sociological and structural analysis of the Polish Jews, although any of the following contemporary studies are excellent and indispensable: Szmuel Hirszhorn *et al.*, *Żydzi w Polsce Odrodzonej* (Warsaw, 1933); I. L. Bronstein, *Rzemięsto żydowskie w Polsce* (Warsaw, 1936);

simulation with the Poles and to retain unaltered their distinctive dress, speech, and dietary and living customs, furthermore, combined with the witty political criticism of such writers as Bernard Singer and Szmuel Hirszhorn, offered a strong temptation to anyone seeking to isolate this group as a possible hostile element within the Polish body politic.⁵ These factors all fostered the growth of an indigenous anti-Semitic movement that most observers agreed was a spontaneous, *sui generis* phenomenon, developing independently of and uninfluenced by events in neighboring Germany, which most Nationalists perceived as Poland's leading foreign enemy.⁶ In order to provide a general framework within which the Endeks' anti-Semitism could be absorbed and any disputed ideological points resolved, the Sanacja leaders advanced the concept of national security against the dangers threatening Poland from abroad as a neutral basis upon which both camps could harmoniously unite.⁷

THE MONTHS FOLLOWING Piłsudski's death witnessed a general rise in disorder across the land, and the Nationalists swiftly seized upon the uncertain conditions to pursue actively their anti-Semitic designs. Throughout 1936 they sparked a resurgence of violent anti-Jewish outbursts that often resembled the classic pogroms of an earlier period; one observer placed the number of Jews killed that year at sixty-nine, with eight hundred wounded.⁸ Reports of atrocities circulated in numerous press articles, holding the attention of the American and British public in particular.⁹ The Polish government sanctified the spread of the trend toward violence by encouraging an economic struggle against the Jews, while officially condemning

Bronstein, "Struktura zawodowa i społeczna ludności żydowskiej w Polsce," *Sprawy Narodowościowe*, 13 (1939): 1-36; M. S. Goldstein and K. R. Dresdner, *Kultura i sztuka ludu żydowskiego na ziemiach polskich* (Lwów, 1935); Adam Prowalski, *Spółdzielczość żydowska w Polsce* (Warsaw, 1933); Władysław Studnicki, *Sprawa polsko-żydowska* (Wilno, 1936); and the excellent statistical work of Bolesław Wasiutyński *Ludność żydowska w Polsce w XIX i XX wieku* (Warsaw, 1925). Almost all of the works in English are colored with the passions of the times, but two relatively objective books are those sections on the Jews in Stephan Horak's *Poland and Her National Minorities, 1919-1939* (New York, 1961), and Simon Segal's *The New Poland and the Jews* (New York, 1938).

⁵ Singer, whose best efforts have been assembled in *Od Witosa do Ślawka* (Paris, 1964), proved especially sarcastic and patronizing when commenting upon the rise of Edward Śmigły-Rydz as Piłsudski's nominal successor. See, for example, his articles in *Nasz Przegląd*, Dec. 29, 1935, and May 18, 1936.

⁶ See, for example, the opinions given in a special report for Poland in 1937 by P. Mathews of H. M. Foreign Office (Records of the Foreign Office [hereafter FO; the FO records are in the Public Record Office, London], FO 371/20760, C 8603/24/55), and the article by Raoul de Craon-Poussy, "Poland and the Axis," *Commonweal*, 29 (1939): 709-10.

⁷ This new rallying cry was first revealed in Rydz's speech to the Union of Polish Legionnaires on May 24, 1936 (*Gazeta Polska*, May 25, 1936).

⁸ Emil Lengyel, "Europe's Anti-Semitic Twins: Poland," *Current History*, 48 (1938): 45.

⁹ Samples of press coverage other than regular newspaper articles are Israel Cohen, "The Jews in Poland," *Contemporary Review*, 150 (1936): 716-23; Abraham Druker, "Jews in Poland," *Current History*, 45 (1936): 62-67; and Druker, "Fight against Ghetto Benches in Polish Universities" and "Ghettos for Jewish Students in Warsaw Colleges," both in *School and Society*, 46 (1937): 502, 591.

acts of violence. Premier Felicjan Sławoj-Składkowski signaled the start of this campaign with his famous speech to the Sejm (parliament) in June, affirming that "of course" (*owszem*) there should be an economic fight for survival between Jews and Poles.¹⁰ Furthermore, the regime accelerated its plans for the mass emigration of Jews to Palestine and other points, heedless of the possible disastrous effects this course of action could have upon Polish economic life.¹¹ The regime soon became alarmed at some of the more brutal excesses, however, and, fearing a degeneration into open anarchy, attempted to curb the Nationalists. Shortly after Składkowski's June speech he announced the arrest of SN leader Adam Doboszyński and several lesser officials; in December he told the assembled Sejm that "a policy that stated that, despite the existence of minorities, there is only one nation in the State and all must work exclusively for it, would be the most popular but would also be a short-sighted policy and a search for some cheap popularity."¹² Abuses against Jewish merchants, students, professional men, and, in many instances, women and children, nevertheless continued unabated throughout 1936.

Midway through the year the Sanacja turned its attention to the actual organization of its new political base. In late May Edward Śmigły-Rydz, inspector general of the armed forces, had forecast the formation of a government front party to rally the citizenry around the regime in a joint effort to "lift Poland higher" through subordinating personal and group aspirations to the needs of the state. Although he expressed the hope that all Poles "sincerely devoted to the Fatherland" would join the new organization, it soon became obvious that the movement would be dominated by those persons traditionally associated with Piłsudski's camp. Rydz entrusted the creation and leadership of his party to Colonel Adam Koc, an eminent Sanacja politician who had become known for his social and political conservatism in the years following World War I.¹³ Jewish spokesmen

¹⁰ *Gazeta Polska*, June 5, 1936. It is ironic that three months earlier the minister of the interior, Władysław Raczkiewicz, had told the Sejm that "anti-Semitism is only a means in the hands of the Endeks, through which they desire to take power in the country." *Ibid.*, Mar. 7, 1936. The Jewish Socialist leader Wiktor Alter offered a rebuttal to the regime in his *Antysemityzm gospodarczy w świetle cyfr* (Warsaw, 1937).

¹¹ For the best concise discussion of Jewish emigration, see Władysław Pobóg-Malinowski, *Najnowsza historia polityczna Polski, 1864-1945*, 2 (2d ed.; London, 1967): 806-21. It should be noted that this solution to the problem had long been advocated by such as the Socialists (Jan Borski, *Sprawa żydowska a socjalizm* [Warsaw, 1937], 7) and the Peasant party, which included it in the program adopted at its congress in 1935. See *Materiały źródłowe do historii polskiego ruchu ludowego*, 3 (Warsaw, 1966): doc. 92. The Jews themselves were not entirely of one mind on this topic: see Adam Tartakower, *Emigracja żydowska z Polski* (Warsaw, 1933); Tartakower, "Jewish Emigration from Poland in Post-War Years," *Jewish Social Service Quarterly*, 14 (1940): 272 ff.; J. L. Ziemiński, *Problem emigracji żydowskiej* (Warsaw, 1937); W. Ormicki, "Warunki i możliwości emigracji żydowskiej," *Sprawy Narodowościowe*, 11 (1937): 280 ff.; and M. J. Pollner, *Emigracja i przewarstwienie Żydów polskich* (Warsaw, 1939).

¹² *Gazeta Polska*, Dec. 22, 1936.

¹³ The speech of Rydz and details of the occasion are in *Gazeta Polska*, May 25, 1936, as well as the collection in the Archiwum Akt Nowych, Warsaw, on the Związek Legionistów Polskich (hereafter AAN and ZLP, respectively), *teczka* ("file"; hereafter *t.*) 24. For a succinct description of Koc's character and political views, see Pobóg-Malinowski, *Najnowsza historia*, 797-98, and

greeted the development with more restraint than did the Polish opposition forces and expressed the conviction that national security could provide the slogan needed to unite the country only if the military-dominated base of the movement were expanded and those economic problems so crucial to this area settled without serious shocks to the country.¹⁴ As the year wore on Koc became involved in maneuvers with the extreme nationalist Falanga of Bolesław Piasecki and continually postponed the formation of the government front party, in the process convincing most Poles that it would be quite conservative in tone when it finally appeared.¹⁵ Meanwhile Jewish uneasiness about the ideological makeup of the embryonic movement was heightened as the new year commenced. Colonel Bogusław Miedziński, the leading parliamentarian of the Sanacja regime and, as editor of the quasi-official daily *Gazeta Polska*, its chief political spokesman, in January 1937 delivered a speech before the Sejm on the problem of Jewish emigration. Declaring that his motives were in no way racist in nature, Miedziński stated that “personally, I love Danes very much, but if we had three million of them in Poland, I would implore God to take them away as soon as possible.”¹⁶ Since Miedziński had assumed the chief role in preparing the Koc group’s platform, Jewish leaders braced themselves and awaited its formal proclamation with, in the words of the British ambassador, Sir Howard Kennard, “not a little trepidation.”¹⁷

When Colonel Koc finally launched the long-anticipated Camp of National Unity (OZN, or simply Ozon) on February 21, 1937, he did little to clarify the regime’s relationship to the Jews. His Ideological-Political Declaration emphasized that every facet of the country’s existence was to be developed by and for “the Polish nation” to the exclusion of the minorities, a thinly disguised call for the thorough Polonization of social, economic, and cultural life, a program that *ipso facto* was directed primarily at the Jews. The declaration went further in singling them out for treatment separate from that accorded Poland’s Christian minorities. After claiming that “the promulgation of racial hatred is alien to the Polish spirit,” Koc outlined his camp’s policy toward the Jews:

We value the level and content of our cultural life—along with order, peace, and quiet, without which no state can function—too highly to be able to approve of any arbitrary action and brutal anti-Jewish outbursts, which demean the dignity and majesty of a great nation; but the instinct of cultural self-preservation is un-

the “Annual Personalities Report for Poland” drawn up by the British Foreign Office, FO 371/20760, C 25/25/55.

¹⁴ *Nasz Przegląd*, May 25, 29, 1936.

¹⁵ See the political dispatches sent back to London from Warsaw by British Ambassador Sir Howard Kennard, FO 19957–64 (1936) and 20759 (1937). When interviewed in London in November 1968, Stahl, who was closely involved in the ideological problems of the new movement, expressed the conviction that these talks with the Falanga were largely responsible for the long delay in founding the party.

¹⁶ *Gazeta Polska*, Jan. 12, 1937.

¹⁷ Kennard to Sir Anthony Eden, Warsaw, Feb. 24, 1937, FO 371/20759, no. 96.

derstandable, and the desire of the Polish community for economic self-sufficiency is natural. This is all the more comprehensible in the period we have just lived through, a period of economic and financial shocks, when only a deep sense of citizenship, self-sacrifice in relation to the State, and an uncompromising bond between one's life and the State can enable it to emerge unweakened from these shocks.¹⁸

This equivocal handling of the matter, reflecting the lack of clear policy lines within the regime, was received with varying degrees of indifference by most of the Polish community. The conservative landowners praised the declaration as being "calm, objective, full of moderation and common sense" and expressed satisfaction with its sections on the minorities as a whole, while their industrialist counterparts ignored this aspect of the Camp's program.¹⁹ The Peasant party (SL) likewise concentrated its attention upon other facets of the OZN ideology, and the Polish Socialist party (PPS) noted with regret that its former comrades, "not many years after the death of Józef Piłsudski, are turning over their brains and skills, their pens and talents to that method of embracing the Polish reality that is represented most accurately and, let us be sincere, most deeply by Roman Dmowski."²⁰ Only the nationalists and their most bitter enemies, the Communist party of Poland (KPP), offered a definite commentary on the OZN's relation to what was becoming known in political parlance as the Jewish question. In October 1936, in an open letter to the Polish people, the Communists had condemned the anti-Semitism so beloved by the Endeks and now being expropriated by the Sanacja as begging the true question, "for, in the face of the great historical tasks presently before Poland, the time-worn phrase that everything is the fault of the Jews won't suffice." In March 1937, the Communist party having itself been designated as Poland's chief enemy, its central committee roundly disavowed Koc's policy of "promoting racial hatred, anti-Semitism, and Jewish pogroms and seeking support among the reactionary clergy and the darkest elements of Polish backwardness."²¹ Both ONR groups avoided specific mention of the Camp's Jewish program, but the SN viewed the entire declaration with overt hostility. *Warszawski Dziennik Narodowy*, the party's press organ, criticized the document as containing "either problems and opinions thus far well known, or else formulas new but very general and ambiguous." Indeed, the Endeks perceived the main flaw of Koc's group to be its treatment of the Jewish question, which, by confining itself to the economic

¹⁸ The declaration was printed in *Gazeta Polska*, Feb. 22, 1937. Details of the Camp's formation are in Tadeusz Jędruszcak, *Piłsudczycy bez Piłsudskiego* (Warsaw, 1963).

¹⁹ For the conservative views see *Czas*, Feb. 23, Mar. 3, and *Słowo*, Feb. 22, 23, Mar. 2, 1937. *Kurier Polski*, Feb. 23, 1937, and *Przegląd Gospodarczy*, Mar. 1, 1937, contain the comments of the industrialists on the Camp's relationship to the Jews.

²⁰ For the SL see *Zielony Sztandar*, Feb. 28, 1937, and the remarks of its exiled leader Wincenty Witos in *Moja tułaczka* (Warsaw, 1967), 393. The Socialist remarks are in *Robotnik*, Feb. 18, 20, 23, 1937.

²¹ *Dokumenty Komunistycznej Partii Polski 1935-1939* (Warsaw, 1968), docs. 36, 50.

sphere, revealed that the regime “does not comprehend that the Jewish question is not limited to economic and cultural matters but is primarily a political problem and that in addition it is the manifestation of a basic, historic transformation in the bosom of the Polish Nation.” All the empty, ponderous verbiage, concluded the SN, exposed either “the mentality of the declaration’s author or the exhaustion of the energy of that group to which he belongs and its incompetence in laying down guidelines for new thoughts, new postulates, and new forms of action.”²²

Stung by this attack from the one political circle it was intent upon wooing, the OZN lashed back at the Nationalists. Miedziński immediately retorted that, as the general framework of a movement that everyone could support, the declaration had to be somewhat vague. Furthermore, if “the Endeks have nothing to say on the important themes and can perceive nothing but Jews, Jews, and more Jews as the main theme of this declaration—this is, indeed, ‘nothing new!’”²³ The SN refused to be drawn into a verbal duel, however, and calmly maintained its critical stance toward the Camp. Its resolve was no doubt bolstered by the actions of what formed the centrist political grouping in Poland, the Christian Democratic party (Chadecja). In March its executive committee adopted a resolution calling for “the removal of the harmful influences of Jewry and Masonry, which cannot be done without the intervention of the State,” and demanding “the solution of the Jewish question through legislation compatible with the principles of Christian justice.” The Christian Democrats concluded by proposing as the only ultimate answer to the problem “the de-Jewification of cities, commerce, industry, and the professions, as well as the removal of those Jewish influences injurious to Polish culture.”²⁴

The Jews themselves initially assumed a cautious tone when discussing the OZN, perhaps relieved that no specific plans had been formulated for them. Singer mildly questioned the true intent of the declaration, reading between those lines that advocated the building up of the Polish middle class and the transfer of the surplus rural population to urban centers and “imagining, with little difficulty, the ordering of life in the cities, especially in those areas where for hundreds of years the Jewish population has formed the predominant portion of the middle class.” He concluded that the OZN platform, coupled with Składkowski’s official proclamation of economic war on the Jews, “contains in itself a program and directives for the administrative elements.” Other writers raised the freedoms of religious and cultural equality guaranteed by the Polish Constitution of April 1935 and asked with concern, “In what manner will the community be able to influence the newly formed progovernment camp

²² *Warszawski Dziennik Narodowy* (hereafter *WDN*), Feb. 22, 1937.

²³ *Gazeta Polska*, Feb. 23, 1937.

²⁴ See the “Monthly Political Report,” Mar. 1937, in the file for Poznań in Centralne Archiwum Wojskowe, Warsaw (hereafter *CAW*), Dowództwo Okręgu Korpusowego (“Regional Corps Command”; hereafter *DOK*), VIII, t. 175a.

in order to produce deeds from this program's words?"²⁵ Generally, however, the Jewish press avoided speaking of the OZN.

Two months lapsed before any further official pronouncements on the Jewish problem released the tensions that had been building in Jewish and nationalist quarters alike. Koc, immersed in the organization of his movement, ignored the Jews until civilian and military authorities throughout Poland began reporting that the political currents in the provinces for the months of February and March were running against the OZN: the Nationalists were not only joining the peasants and Socialists in opposing the Camp but often directed the attacks themselves.²⁶ Faced with the complete collapse of his entire strategy before it had been fully developed, Koc ordered his recently appointed chief of staff, Colonel Jan Kowalewski, to hold a press conference devoted to sketching the organization's progress. To no one's surprise the Jewish question occupied a major portion of the proceedings when this urbane Pole met with his interrogators on April 20. Noting that this issue was "very familiar to all of us" and posed "one of our chief problems," Kowalewski offered a concise statement of the Camp's stand: "However much we oppose all excesses in this land and will struggle against them, we shall aim simultaneously at an organic solution to this matter." He replied to a question concerning Jewish eligibility to join the OZN by asking whether a Pole could or should belong to the Zionist party, asserting that "it is obviously essential to comprehend the concept of being a Jew as one of nationality. A Pole can also be of Catholic, Muslim, or Mosaic faith. All Poles are permitted to join the Camp, irrespective of their religious origins or race—but only Poles." This rather unusual explanation apparently pleased no one, for the following day *Gazeta Polska* printed an in-depth interview with Kowalewski devoted exclusively to the Jewish question. Asked whether a person of Jewish origin and Mosaic faith could belong to the OZN if he considered himself to be a Pole, the chief of staff unequivocally announced that "the principles of Christianity, on which the declaration of Colonel Koc rests, will be the decisive factor in selecting members." He then defined the claim of belonging to the Polish nation as being "not only an avowal of nationality but also a sacrifice of blood, voluntarily shed, or other proof of sacrifice laid down on the altar of the Fatherland and the actions of an entire lifetime that testify to genuine membership in the Polish Nation." Referring to the considerable number of Jews who had served under Piłsudski, Kowalewski admitted their valor but regretted that "it is a simple fact that they cannot belong to the Camp of National Unity." In conclusion he stated that emigration

²⁵ *Nasz Przegląd*, Feb. 22, 23, 1937.

²⁶ The civilian reports are found in the ministry of the interior (MSW) file in the AAN, t. 853, and the military in the Poznań file of the CAW cited above, n.24, as well as the papers for the Lublin district (CAW/DOK II, t. 143).

was not the only solution to the problem and pledged his party's support of a program that would make it possible "for the Polish population to find employment in the trades, industry, and commerce, so that these three spheres of economic life might become Polish and that Polish cities could play their proper cultural as well as economic role."²⁷

Reaction to this new declaration of policy was swift and, unlike that recorded in February, forthcoming from every segment of Polish public opinion. Stanisław Cat-Mackiewicz, the acid-tongued owner and editor of *Słowo* and self-styled spokesman of the magnates in the Lithuanian regions of Poland, promptly expressed his approbation of the new stand as bringing a welcome "relaxation" in the matter and being "in many cases a cruelty, but a necessary cruelty."²⁸ The industrialists were visibly more restrained in their appraisal of the new move, warning against drawing hasty conclusions from the apparent similarities between the OZN and Endek programs, for "the Jewish question is not, it seems to us, sufficient basis for a political understanding between the OZN and the SN, the most opposition-minded of the Polish political parties."²⁹ The centrists greeted the "clarification of the situation" as a positive step forward for Koc, but the massive SL, often moderately inclined, sneered at the regime's growing anti-Semitism as merely a feeble excuse to perpetrate Poland's real evil, "the gentry exploitation directed against the peasants."³⁰ The Socialists likewise voiced grave doubts over the course the regime seemed determined to pursue. Mieczysław Niedziałkowski, editor of the PPS organ *Robotnik*, perceived that "a Sanacja-Endecja is being created" behind a screen of ponderous verbiage and labeled the Camp's position "the obvious denial of all the work of Józef Piłsudski as Head of State in the years 1918–1922 and thus the triumph of the concepts of Roman Dmowski."³¹ The consequences for Poland and the Socialists of a Sanacja rapprochement with the Endeks were well realized by the PPS, whose May Day slogans drew attention to this danger and whose Rada Naczelna (grand council), meeting on May 9, passed a special resolution pointing out that "the pressure of reaction is growing; the old Sanacja camp is breaking up now that the so-called OZN is being formed, in which all nationalist elements are supposed to come together and unite under slogans promoted by the Endeks and not so long ago fought by the Sanacja."³² The PPS central committee joined the Jüdischer Arbeiter Verband (Bund) in releasing a statement on

²⁷ *Gazeta Polska*, Apr. 21, 22, 1937.

²⁸ *Słowo*, Apr. 22, 1937.

²⁹ *Kurier Polski*, Apr. 29, 1937.

³⁰ The moderate nationalist wing of the centrists spoke through *Kurier Warszawski*, Apr. 25, 1937, while the SL remarks came in *Zielony Sztandar*, July 11, 1937.

³¹ *Robotnik*, Apr. 22, 23, 25, 1937.

³² For the slogans, see "Circular No. 2 of the CKW [Centralny Komitet Wykonawczy, "central executive committee"] PPS (30 III 37)," AAN/MSW, t. 876. The resolution was printed in *Robotnik*, May 11, 1937.

June 9 decrying the use of anti-Semitism as a political tool, and the head of the Socialist trade-union organization followed suit in a similar declaration.³³

To the regime's chagrin the Endeks again found serious shortcomings in its treatment of the Jews. The Nationalists felt that Kowalewski had merely rehashed old ends without mentioning any new means; while such platitudes were understandable in light of the Sanacja's past habit of cooperating with Jewish parliamentarians in the defunct Nonpartisan Bloc for Cooperation with the Government (BBWR), they "do not appear too promising" when compared to the problem then before them. The proper conclusion presented a gloomy enough picture to the SN:

We must state that the position of the Camp toward the Jews as outlined thus far does not answer in full either the aspirations of the Polish community or the latest phase of this problem. From what is known of the OZN's relation to the Jews, we can conclude that, in comparison to the BB[WR], certain progress is undeniable. But in comparison with the increased tension of the Jewish question and the practical results of the struggle for the Polish character of the country—particularly concerning the need to awaken our community to this problem—it is scarcely teething.³⁴

In fact, Kowalewski's performances led the Endeks to believe that the regime's leaders had absolutely no conception of how such a political body as the OZN should be run.³⁵ Two days after its editorial denunciation of the Camp the SN executive committee adopted a resolution in which it condemned the actions of both Koc and the government toward the Jews as being far too lax in view of the seriousness of the problem and expressed regret at the manner in which the army had become estranged from the community by being used as the pawn in a political game.³⁶ When Stefan Starzyński, mayor of Warsaw and head of the OZN urban sector, defended Jewish constitutional rights at a nationwide gathering of city officials on April 30, the party triumphantly exhibited the event as proof of the Camp's true feelings toward the Jews and hence as evidence of the deception that marked its entire program.³⁷

Nor was this negative view limited to strictly Polish opinion. The UNDO, chief political organization of the volatile Ukrainians, defiantly proclaimed that "we go to the side of the Jews, for purely political as well as cultural humanitarian reasons, as to a natural ally in the existing conditions."³⁸ The mood of great powers as well was hostile to the trend that develop-

³³ *Robotnik*, June 9, 12, 1937. For a general history of the *Bund* see Bernard Johnpoll, *The Politics of Futility: The General Jewish Workers Bund of Poland, 1917-1943* (Ithaca, 1967).

³⁴ *WDN*, Apr. 23, 1937.

³⁵ *Ibid.*, Apr. 22, 1937.

³⁶ A copy of this resolution, which was seized by the ministry of the interior, was sent to London by Kennard (Warsaw, May 19, 1937, FO 371/20759, no. 257).

³⁷ *WDN*, May 1, 1937.

³⁸ *Dilo*, June 14, 1937.

ments in Poland seemed to be following. The British made known their displeasure in both press and diplomatic reports, and the Polish ministry of foreign affairs received warnings from its own representatives in London that the official anti-Semitism was seriously damaging Polish prestige.³⁹ The *New York Times* devoted a lengthy editorial to the Camp, calling it “not only strongly nationalistic and authoritarian” but also “more and more racist and exclusivist, so that Poland, the oppressed and divided land freed in the name of liberty and self-determination, is being remade in a spirit terribly like that of Nazi Germany.” The net effect of this policy could only be disastrous for the country, predicted the *Times*, for “the spread of racial intolerance alienates from Poland the world sympathy which more than anything else won the Poles their independence.”⁴⁰ The French and Soviets offered no commentary on the issue, while the Germans, however delighted they may have been with events in Poland, were forced to cast an anxious eye on the rights of their own nationals within that country and hence preserved a judicious silence in the press.⁴¹ Most important, the Camp’s behavior not only had failed to gain Nationalist approval but had succeeded in arousing the political consciousness of the hitherto soft-spoken Jews. Singer angrily noted that the two performances of Kowalewski “left no doubts” as to how Koc’s declaration would be implemented, and he voiced the hope that Poland’s Jewish population would realize “that the defense of their questioned basic rights to exist should be waged on a basis of absolute solidarity, because no differences dividing the Jewish community can become obstacles in this united struggle.”⁴²

Its reliance upon anti-Semitism having proved ineffective, the regime then turned to the youth sector as a possible means of drawing together the two hostile camps. In June Koc announced the formation of the Union of Young Poland (ZMP) as the Camp’s official youth movement and entrusted its direction to Piasecki’s chief lieutenant. The SN proved as antagonistic to this move as they had to the declaration and overtly ignored the new group.⁴³ Despite this attitude of nonrecognition, the Endeks could not resist the temptation to differentiate the “certainly sincere” anti-Semitism that the ZMP was energetically promoting from the “philo-Semite” mood of

³⁹ See, for example, articles in the *Times* (London), Mar. 23, Apr. 3, 23, 27, and July 21, 22, 26, 1937. See also the political dispatches from the Warsaw embassy in FO 371/20759, C 3092, 3282, 3703, 5887/24/55, and FO 371/20760, C 7046, C 8603/24/55. Jan Szembek, undersecretary for foreign affairs, recorded a report from a Polish embassy official that stressed the negative response aroused in England by the Camp’s anti-Semitism. Tytus Komarnicki, ed., *Diariusz i teki Jana Szembeka*, 3 (London, 1969), 87.

⁴⁰ *New York Times*, June 12, 1937.

⁴¹ See, for example, *Berliner Tageblatt*, Mar. 6, 1937, and *Völkischer Beobachter*, Mar. 7, 1937. See also *New York Times*, Feb. 23, 1937.

⁴² *Nasz Przegląd*, Apr. 22, 30, 1937.

⁴³ Sarcastic references to the ZMP appear in *WDN*, June 25, Aug. 3, 1937. See also the official statement released by the department of press and propaganda in the SN central committee on August 5, 1937, in AAN/MSW, t. 866.

the older followers of Piłsudski and to see this difference as further evidence of the "hypocrisy" of the Camp's doctrines.⁴⁴ The failure of the OZN to proceed beyond the turning of ambiguous phrases in the Jewish question, meanwhile, had begun to arouse the impatience of many Poles. The caustic Mackiewicz, frustrated in his desires to see the Camp purge the Sanacja of any leftist and centrist elements and establish itself as a right-wing government, in September angrily cried out: "Enough of this exaggeration of the Jews. . . . Let the Ozon either return their dues to the present members or cease to be anti-Semitic!"⁴⁵ Faced with the growing alienation of many former supporters, Koc permitted the Falanga-dominated ZMP to convert its verbal anti-Semitism into deeds. Violent excesses against Jews returned to the Polish scene, thereby crystalizing the more liberal opposition within the regime, which had been slowly gathering its forces against the Koc-led affiliation with the extreme nationalists. A group of dissident officials broke off from the Sanacja in October 1937 to form the Democratic Club, whose initial resolutions strongly castigated the OZN for its chauvinistic nationalism and anti-Semitism.⁴⁶ Earlier, with pressure against Koc steadily mounting, the police had conducted a major round-up of Falangists in connection with a wave of anti-Jewish bombings and riots. This in turn precipitated a reaction that, after two months of tension throughout Poland, resulted in Rydz's promise to steer the regime along a "middle course" and the public repudiation of the ZMP's organizational monopoly in the OZN youth movement.⁴⁷

By mid-January 1938 General Stanisław Skwarczyński had replaced Koc as head of the OZN, prompting widespread speculation that the regime's hypernationalist evolution would be abandoned. This possibility appeared heightened the following month when Dr. Zdzisław Stahl, incensed at the unending stream of criticism aimed at the Camp by his former comrades, accused the Endeks of "Judeocentrism," of using the Jewish question as the focal point for all their policies in an attempt to conceal their own ideological poverty.⁴⁸ Whatever hopes the Jews may have harbored for a relaxation in official anti-Semitism, however, were rudely shattered by Skwarczyński's major policy speech on February 21.⁴⁹ Referring specifically to the

⁴⁴ *WDN*, Sept. 8, 20, 1937.

⁴⁵ *Słowo*, Sept. 24, 1937.

⁴⁶ Printed in Leon Chajm, ed., *Materiały do historii Klubów Demokratycznych i Stronnictwa Demokratycznego w latach 1937-1939*, 1 (Warsaw, 1964): doc. 18. See also Chajm, "U źródeł powstania Klubów Demokratycznych," *Najnowsze Dzieje Polski*, 11 (1967): 129-58.

⁴⁷ No specifics were ever made public on a secret meeting held on October 30 between Rydz and the leaders of the ZLP, but the following accounts all more or less agree on detail: *WDN*, Nov. 6, 1937; *New York Times*, Oct. 31, 1937; *Völkischer Beobachter*, Nov. 2, 1937; and the memoirs of leading Sanacja financier Henryk Gruber, *Wspomnienia i uwagi, 1892-1942* (London, 1968), 373-74. Koc repudiated the ZMP's ties with the *Falanga* in an interview in *Gazeta Polska* on October 28. See also Pobóg-Malinowski, *Najnowsza historia*, 804, and the first edition of this work, vol. 2, pt. 1 (London, 1957), 613.

⁴⁸ *Gazeta Polska*, Feb. 17, 1938. By this time Stahl was an assistant editor of *Gazeta Polska* and emerging as a leading Sanacja ideologue.

⁴⁹ *Gazeta Polska*, Feb. 22, 1938.

Jews, he declared that "through their unique demographic structure they pose an obstacle to the normal evolution of the masses of the Polish nation," a fact that "must arouse feelings of animosity between the Jewish and Polish populations." Its leader promised that the Camp, "opposing all demagogic and irresponsible terrorist activity against the Jews as harmful and impeding the harmony of the nation," sought to solve the problem with "a radical decrease in the number of Jews in Poland," for "the assimilation of the Jews is not a goal of Polish nationality policy." The Jewish press, perceptibly less restrained since Koc's downfall, responded to this speech instantly. *Nasz Przegląd* attacked it as advancing fallacious postulates for the unreal situation Koc had produced and voiced dismay that the regime was now developing a genuine program to deal with the Jews instead of mouthing slogans and phrases.⁵⁰ Hirszhorn interpreted the contradictions within the movement as symptomatic of the deepening conflict between the Right and Left for control of the Sanacja and concluded that "thus far the compromise has not succeeded."⁵¹

In the ensuing two months the forces of the Left appeared to have prevailed. Both the ZMP and the extreme proregime nationalists associated with the publication *Jutro Pracy* were purged from the OZN, moving the SN to blame the uproar on "Jewry, with its numerous affiliations and subordinate organizations, which have had deep roots in the Sanacja regime for a long time," and allowing them to gloat that "in a word, the attempt at exploiting the Endeks for their slogans backfired and ended in complete failure."⁵² Despite Skwarczyński's assertion that "I always was, am, and will be an advocate of healthy and positive nationalism, having as its goal the good of the Polish Nation and State," the leftist opposition was openly elated at the decline of the nationalists.⁵³ In a series of editorials Hirszhorn analyzed these upheavals as demonstrating that "what is currently transpiring in the Ozon under the leadership of General Skwarczyński appears to be a return to the old Piłsudski tradition . . . a return, however, for the moment cautious and halfway, whose success is difficult to predict. Meanwhile, we see a definite ebb in the old clan's rightist sympathizers."⁵⁴ When the Rada Naczelna, newly created to serve as the Camp's chief body for policy formulation, was staffed mainly with members of the regime's liberal Naprawa group, prospects seemed auspicious for the resumption of normal political relations in Poland.

It soon became apparent that the supremacy of the Left within the regime had been attained at the price of a compromise with its reactionary ele-

⁵⁰ *Nasz Przegląd*, Feb. 23, 1938. Concise translations of that segment of the Jewish press written in Yiddish are on file in AAN/MSW, t. 964.

⁵¹ *Ibid.*, Feb. 24, 1938.

⁵² *WDN* issues of Apr. 21, 22, 23, 25, 1938, all cover the crisis.

⁵³ Skwarczyński's declaration came in a special interview printed in *Gazeta Polska*, Apr. 27, 1938.

⁵⁴ *Nasz Przegląd*, Apr. 22, 1938. See also issues of Apr. 26 and May 4, 1938, as well as AAN/MSW, tt. 965 and 966, for summaries and analyses of the Jewish press for this period.

ments, who were rewarded with the revival of anti-Semitism in a more lethal form. Prior to Skwarczyński's speech the new chief of staff Colonel Zygmunt Wenda, known for his dogmatic nationalism, had released to all of the Camp's activists a confidential circular stating that "the activity of the Polish community in the economic sphere must be consistently and determinedly conducted, and the OZN's outposts should lead in this."⁵⁵ Shortly thereafter posters began to dot the countryside and the Polish sectors of cities, arousing racial as well as economic animosity toward the Jews and calling upon all Poles to take up the anti-Jewish crusade.⁵⁶ This unexpected offensive moved the Jews to caution the regime that "it is impossible to remove the Jew from one profession and not permit him to work in another without retribution, for Jews are not only sellers but consumers as well; hence, whoever economically destroys the Jewish merchant, artisan, or white-collar worker simultaneously destroys a consumer who is served by an entire rank of Christians, primarily peasants." In these circumstances, stressed *Nasz Przegląd*, "if the consolidation of which the Ozon dreams is to be creative, it must be based on harmony, not strife." By now thoroughly alarmed at the turn events had taken, Jewish spokesmen used the anniversary of Piłsudski's death to remind his successors that his heritage "does not leave the least doubt regarding its relationship to the mass of citizens. The spirit of any kind of discrimination is alien to it. There emanates from it the command to recognize rights and merits while condemning illegalities—without any discriminatory qualifications."⁵⁷

Heedless of these admonitions the Sanacja moved to provide its anti-Semitism with the ideological basis that thus far had been conspicuously lacking. During the course of 1937 the OZN bureau of planning had compiled for future guidance a "strictly confidential" study on the state's relation to the Jews, the main points of which Skwarczyński had incorporated into his February speech.⁵⁸ The new Rada Naczelna devoted the major portion of its inaugural session in late May to the further development of these principles into a discernible body of doctrine. Its *Theses on the Jewish Question*, and their subsequent elaboration by Miedziński in *Gazeta Polska* over a two-week period, constituted the official program of the Polish authorities on the Jewish question and hence merit closer examination.⁵⁹

THE STARTING POINT for the Camp's treatment of the Jews "as a political factor" was stated to be "their membership in a universal, Jewish a-state

⁵⁵ "Instructions for Activists and Members of the OZN," Feb. 1938, AAN/OZN, t. 17.

⁵⁶ *Ibid.*, t. 77, contains some sample posters. Typical slogans were "Buy Only in Polish Shops!"; "Each New Polish Shop is an Outpost in This Struggle!"; and "A Poland Free from Jews is a Free Poland!"

⁵⁷ *Nasz Przegląd*, May 10, 12, 1938.

⁵⁸ "Information Prepared by the Bureau of Planning: 'The Position of the OZN in the Jewish Question—Strictly Confidential,'" 1937, AAN/OZN, t. 12.

⁵⁹ Both the theses and commentaries, all published separately in *Gazeta Polska*, May 22, 25, 26, 27, June 4, 9, 12, 1938, were released in brochure form by the OZN as an official program addendum. See Bogusław Miedziński, *Uwagi w sprawie żydowskiej* (Warsaw, 1938).

group [*pozapaństwowa grupa*] possessing separate national goals." The Rada explained that "the effect of these separate political aspirations and the effect of their numbers, plus their major influence over many areas of social and national life, is to make the Jews, in the present state of affairs, an element that weakens the normal development of national and state strength that is currently being achieved in Poland." To these political, economic, and social considerations Miedziński added a fourth, "of uncommon importance, separating the Jewish question from all other nationality problems"—religion. Since, alone of Poland's inhabitants, the Jews did not have to abide by the Christian ethic, their religious differences were thus transformed into moral and ethical ones as well. "It would therefore be impossible," wrote Miedziński, "to deny the reality of the effects of such a phenomenon as the ethico-religious distinctness of the Jewish masses, a distinctness that, in relations with individuals of another nationality, wipes out any criteria of a moral nature, leaving the risk of being penalized as the sole moralizing factor." He viewed this situation as being the key to Polish anti-Semitism and emphasized that denying the importance of this distinction "would be closing one's eyes to reality." There were yet other qualities that set the Jews apart as an ethnic group, the most important being their customs. Miedziński complained of the offensiveness to both Poles and Europeans of the "hopeless dirt and slovenliness" found in the Jewish quarters of medium and large Polish cities, conditions that "lower the level of our culture in our own eyes and those of foreigners." This lack of cleanliness was minor in comparison with other clashes of mores, however:

They are based, in short, on the careful cultivation of, to the eyes of a modern European, disgusting and ridiculous sorcery and superstitions, recalling the practices of Asiatic shamans or black chieftains from the African jungle; for example, let us mention ritual slaughter [of animals]. These are cultural differences of such a magnitude that in general it is impossible to compare them with differences in custom existing, for example, between the Russian and Polish peoples.

These religious, ethical, and cultural differences all combined with "Jewish nationalism" to raise the red specter before Miedziński's concerned eyes. At the very moment that Stalin was purging his land of many of its Jews, it struck Miedziński as hardly surprising that the Jews in Poland, surrounded by poverty and beholding the key roles played in neighboring Soviet Russia "by the Zinovievs, Trotskys, Kaganoviches, Litvinovs, etc.," should have their national pride aroused. "From the year 1918," stated the colonel, "we have observed the undeniable cooperation of a significant portion of Jews, especially the young, with our enemy, which was the Red Army in the course of the early war years but from the conclusion of peace to this day is the Comintern." As clear proof of this tendency he cited the directory of the KPP, which was ninety per cent Jewish.

The final consideration—economic—had a psychological as well as prac-

tical side. Miedziński claimed that the Jews, with their a-state disposition, were "inclined to panic and psychosis" and thus severely undermined the masses' confidence in the government by withholding Jewish money from circulation during times of stress. "Here is a heavy stone around the neck—incomparably heavier than would follow from the percentage of Jews in the land," for "the participation of this population in money exchange, the disposal of capital, and the shaping of this very 'climate' grows incalculably higher." The regime was thus forced to be seriously concerned with the vast mass of Polish Jewry:

The internal arrangement of forces and differences within this mass are of secondary importance for us. The Communist part of Jewry is the open enemy of our Nation and State; the conservative portion is, through its cultural and ethical differences, a heavy burden upon our national and state life. It is a foreign body, dispersed in our organism so that it produces a pathological deformation. In this state of affairs it is impossible to find a way out other than the removal of this alien body, harmful through both its numbers and its uniqueness.

The *Theses* themselves, with a wary eye on the aggressively anti-Semitic nationalists, openly condemned "activity of a demagogic and anarchistic nature" as an attempt at making the problem into "an instrument of political-party strife" and called instead for "a planned solution by state and local agents." At this point the Rada introduced an important new argument: "The present high rate of Jewish participation in the various professions should be reduced . . . through the introduction of general legal regulations permitting the possibility of selection from the viewpoint of the State's best interests." The Polonization of cities and culture, including schools, was again mentioned, and any likelihood of assimilation was dismissed. On the last point Miedziński faced a dilemma. On the one hand, insofar as the Jewish masses were concerned, "the history of the seven-century existence of this population in Poland clearly shows that this mass is not assimilating"; hence the line to be followed in relation to these Jews was obvious. Far more difficult was the role of the Jewish intelligentsia, many of whom had fought alongside Poles for independence and then cooperated fully in building up the Polish state and nation. "This matter," admitted the colonel, "has all the attributes of a moral problem." He initially left it largely unresolved, temporarily ending the discussion with an assurance that the Camp had indeed remembered these men when formulating its theses and with the defiant assertion that the nonassimilation policy "reflects the actual state at the moment, as accepted by the vast majority of the Polish Nation." Miedziński did point out, however, that one thing had to be understood—the Polish professions had to be opened to worthy Poles:

We state, openly and sincerely, that not even the most lofty theories can convince us that it is necessary to accept this present state of affairs with passive submission. For it opens before our eyes the prospect that, after the lapse of a certain number of years, we would become a nation of a very strange construction . . . a nation of

Polish workers and peasants, directed by an intelligentsia of a different national origin. No doctrine and no world outlook [*światopogląd*] can still in us the instinctive struggle against this prospect.

Shortly thereafter he concluded that an ultimate solution would be possible only when the Jewish and Polish intelligentsia could find a common basis for communication. Since "there are problems, frequently of the utmost urgency, . . . in which it is impossible to find a common language with the intelligentsia of Jewish origin, although they love the Polish land as much as we do and can write and speak of it more beautifully than can we Poles," Miedziński reluctantly admitted that the only feasible answer was their forced emigration. This approach, together with an economic offensive against the Jews, was "the only proper method" of settling the Jewish question. He concurred in the Rada's rejection of demagoguery, violence, and terror as incompatible with "the outstanding merits of the Polish Nation," which he defined as "chivalry" and "adherence to the Christian ethic." Miedziński exhorted his readers to remember that the Jewish and Polish ethical systems were different and that "hence those methods in the struggle with the Jews that lead to our imitation of the relativity of ethical norms are highly dangerous" for "when we stand on the position that, in reference to the Jews, those methods inconsistent with the principles of our morality are permissible, this is a slippery road. Principles once broken lose their binding strength." The colonel terminated his treatment of the Jewish question by insisting that in view of its moderation the OZN was perfectly justified in demanding "an absolutely loyal attitude in relation to the needs of the Polish State and Nation from the side of the Jewish masses and their leaders" while both the Camp and the government worked out the answers to the riddles before them.

In conducting these intricate discussions of the role of the Jews in Polish politics Miedziński (himself married to a Jewess) and other Sanacja officials went to great lengths to explain that they were in no way motivated by racist or discriminatory ideas. In publicly refuting the charge of racism leveled against the *Theses* and his elaboration of them, Miedziński further revealed the nature of the regime's stand:

Is racism or chauvinism hidden beneath these words? No. Between positive nationalism, which we invoke, and chauvinistic racism, which we reject, lies the same difference as between healthy pride and a sense of national dignity, and assigning to oneself or one's nation a supernatural predestination to rule over others. . . . Rejecting racism, whose principal attribute is the thesis of the supremacy of a given national virtue over others, we nevertheless stand on the position that the distinctiveness of a nation's culture is an immeasurably high value that must be defended and developed. The problem of the distinctiveness of a national culture has nothing in common with racism, and in particular with anti-Semitism.

At any rate, by the end of 1938 the *Gazeta Polska* could proudly declare that the OZN's policy outlines, whatever their origins, had become the official

government program for resolving the fate of the country's Jewish population.⁶⁰

The Polish community easily perceived the importance of this latest stage in the evolution of the regime's anti-Semitism. Although the Endeks ridiculed the theses as being pathetically insufficient to deal with the problem and, in fact, a splendid example of that very sin of which the OZN had accused the SN—the utilization of this crucial matter to serve a purely tactical political end—their criticism had mellowed noticeably over previous commentaries.⁶¹ The more radical ONR-ABC group categorized the resolutions as “very lengthy and cautious.” The main significance of the resolutions was as a sign that the leftist faction in the regime, having rid itself of whatever nationalist elements it may have once possessed, now realized that nationalism was the only appeal to which the Polish community would respond and that, therefore, “in theory, speech, and spirit it is essential to be decided nationalists.”⁶² The peasants and centrists pointedly neglected the development, but the Socialists expressed grave concern over the theses, which Niedziałkowski felt had “considerable political significance.” The party warned that the regime was traversing “a dangerous road that could easily lead to that Judeocentrism of which *Gazeta Polska* not long ago accused the Endeks” and sardonically voiced its relief that, in a time of such international strain and uncertainty, the OZN had correctly pinpointed the Jews as the root of all the country's difficulties.⁶³

The regime's open challenge galvanized Polish Jewry into action and brought forth the most determined show of Jewish resistance yet seen. The program that emerged from the Rada's session “is not a surprise for us Jews,” stated Hirszhorn; the former leader's generalities had to be given some substance “to show their conservative-nationalist opponents that nothing has changed since the resignation of Colonel Koc.” In addition to being economically impractical and morally deplorable, such an emphasis upon national separatism could have extremely damaging repercussions for Poland because “the national movement is developing not only among Jews, but also among other minorities, of which many show further-reaching separatist tendencies than the Jews. . . . The proclamation of the principle that whoever does not belong to the ruling nation must be restricted in his rights and forced to leave the country will render Poland's tasks difficult in taking care of the Ukrainian, German, and Belorussian questions and will place Poles in a troublesome position in other countries.” Hirszhorn pointed out that, owing to a dearth of funds, mass Jewish emigration was temporarily out of the question, but he warned his government that, if it still envisioned this means as a viable solution to the Jewish problem, its

⁶⁰ *Gazeta Polska*, Dec. 19, 1938.

⁶¹ *WDN*, May 23, 1938.

⁶² *ABC*, May 24, 28, 1938.

⁶³ *Robotnik*, May 24, 27, 28, June 2, 1938.

spokesmen must remember that “the more anti-Semitism grows in one country, the more it will also spread in other lands, a trend that will render emigration a near impossibility.”⁶⁴ The Jewish Parliamentary Circle released a unanimous denunciation of the theses, attacking their basic premise as “in itself unreal, incorrect, and false, incompatible with the role and importance of each national minority in the State,” and thus a line of reasoning that “will lead to the disaffection of that minority and, through that, to the weakening [of the state] and a decrease in its national security.” The legislators further complained that “the application of such a premise, in a quite incomprehensible manner, solely in reference to a Jewish minority having no territorial demands to make on the Polish State, is obviously only a hypocritical pretext for depriving the Jewish population of its rights as citizens guaranteed by the present Constitution, for denying the Jews an economic existence, and for degrading them to the role of a dispensable element.” They promised that they would fight against the legislative realization of this program, which they characterized as “drawn from foreign models and based on the ideology of brute force and, as such, far removed from the principles of law, morality, and humanity.” They ended their proclamation with the strongest response to the regime ever made publicly by Poland’s Jews:

We state that the Jewish population in Poland will not yield before lawlessness; will not resign from its rights as citizens, guaranteed by both the March Constitution and the existing one; will not resign from the possibility of a cultural, social, and economic existence; will not permit themselves to be reduced to the role of helots or parasites; but, in support of their solid, indestructible forces, in full consciousness of their obligations to the State and through their dedicated fulfillment, will fight indefatigably for full legal equality, for the strict execution of not only the letter but also the spirit of the Constitution of the Polish state in relation to its three and a half million citizen-Jews. The Jewish population is deeply convinced that the ONRist conception of the Jewish question in Poland, which so broadly manifests itself in the above-mentioned theses, will not take in the broad strata of the Polish Nation.⁶⁵

The other Jewish papers joined *Nasz Przegląd* in advancing these or similar sentiments while discussing the OZN program over the succeeding two weeks.⁶⁶ On May 25 the central committee of the Bund added its voice to the cacophony of abuse raining down upon the Sanacja by passing a resolution deriding the theses, and prominent rabbis publicly assailed Miedziński as a racist. It remained for the witty pen of Singer to observe that “anti-Semitism is presently becoming the property of the State and, like every state enterprise, is going bankrupt.”⁶⁷

⁶⁴ *Nasz Przegląd*, May 24, 1938.

⁶⁵ *Ibid.*, May 25, 1938.

⁶⁶ *Ibid.*, May 26, 27, 28, 1938, and AAN/MSW, tt. 966–67, for translations of the Yiddish press. Other leading publications were *Unzer Ekspres*, *Hajnt*, *Folkscajtung*, *5-ta Rano*, *Nowy Głos*, *Moment*, and *Dos Judische Togblat*.

⁶⁷ *Nasz Przegląd*, May 30, 1938. For the CKW Bund resolution see AAN/MSW, t. 966, no.

With a renewed rise in tensions within the regime, however, the Jewish question disappeared from official view until the autumn campaigns for the parliamentary elections revived it as a topic of political value. The Jews initially decided to await the moves of the Polish opposition before committing themselves to a course of action, in the hope that a concerted anti-OZN drive would be mounted. When it became obvious that the major parties would repeat their 1935 boycott of the balloting in protest against the existing electoral system, *Nasz Przegląd* summoned its readers to participate in the voting as a demonstration of their political consciousness and love of the state.⁶⁸ Mackiewicz reported from Wilno that the OZN had cynically taken advantage of this decision by plastering the ghetto area with Yiddish-language posters urging the Jews to vote for Skwarczyński.⁶⁹ The Peasant party tartly noted that, although the Camp stridently promoted its anti-Semitism, in the rural areas "the Jews are indeed the element supporting the newly elected Sejm, for most of them went to vote, freely or under compulsion, for the Ozon candidates."⁷⁰ The Jews were compensated for their cooperation with five seats in the Sejm and one in the Senate. Immediately following the contest the ONR-ABC leadership reflected that, with the parliament now dominated by the government party, they could reasonably expect some concrete action on the Camp's Jewish program, for "the Ozon will have to realize these postulates or else give up their policy as developed thus far." Chiefly to be anticipated were bills abolishing ritual animal slaughter, restricting the operations of the Masonic movement, and "enacting a series of laws regarding the Jewish question."⁷¹

But the regime was mainly concerned with the forthcoming local elections, in which the opposition parties would participate in full, and devoted its energies to preparing strategies for the major urban centers. Not unexpectedly, the Jews assumed special importance as a campaign issue. On the eve of the municipal elections in December the SN charged that, because the Camp refused to understand that this problem was first and foremost a political matter, "the position of the OZN is outdated and its recommendations insufficient" and that therefore its stance "does not even arouse fear among the Jewish community." The Endeks claimed that between their program and that of the Sanacja there existed "not only a quantitative difference but a qualitative one as well"; this inevitably meant that coming local elections would have a political character in that the positions of future city councils on the Jewish question would be at stake.⁷²

12. Miedziński mentioned the public declamations against him when interviewed in London on November 22, 1968.

⁶⁸ See *Nasz Przegląd*, Sept. 16, Oct. 22, 24, 1938, on the Jews' relationship to the elections.

⁶⁹ *Słowo*, Dec. 1, 1938. The article was confiscated by the ministry of the interior when it initially appeared in October. Mackiewicz finished behind both Skwarczyński and the top vote-getter in Wilno, General Lucjan Żeligowski. See the Jewish comment on this incident in *Nasz Przegląd*, Mar. 4, 1939.

⁷⁰ *Zielony Sztandar*, Dec. 4, 1938; see also the issue of Mar. 19, 1939.

⁷¹ *ABC*, Nov. 8, 10, 1938.

⁷² *WDN*, Dec. 6, 1938. See Stahl's reply in *Gazeta Polska*, Dec. 10, 1938.

The regime, which had been advertising these contests as apolitical in nature, was forced to discard that approach in responding to the Nationalist challenge. As the leader of the majority party, Skwarczyński opened the Sejm on December 3 with a lengthy speech in which he made the usual allusions to the Jews, pointing to the declaration and the *Theses* as providing “both the directives for our parliamentary work and our legal initiative.” The first sample of the former came three weeks later in the immediate aftermath of the major elections when the Camp’s chief delivered a formal interpellation to the government on behalf of the 116 other OZN deputies. The state-backed lists had performed well below expectations, and the opposition was in a jubilant mood. After again rejecting violence but demanding planned emigration procedures and “the Polonization of our industry, commerce, and skilled crafts, as well as the elimination of Jewish influences from Polish cultural life,” Skwarczyński asked “whether the Government intends to take up immediate, energetic, and multilateral activity having as a goal the broadest possible reduction of Jews in Poland through the use of all available means.” This was promptly hailed by *Gazeta Polska* as an “important initiative” that would precipitate “an absolute turnabout in the handling of the Jewish question after which, instead of being a burning center of, and an occasion for, partisan struggles among Poles, it should become the object of consistent, constructive action.”⁷³

The country did not share in this official enthusiasm for the OZN maneuver. The SN branded the interpellation as incomplete and hence ineffective, stating that “as long as real measures instead of solemn declarations on the Jewish question do not come from the OZN, we will have to treat the anti-Semitic phraseology of our opponents with a lack of faith.”⁷⁴ The ONRists interpreted the interpellation as clear evidence that the regime was unsure of itself, for, while it was a positive step insofar as it reflected a growing awareness of the problem’s existence, “it is only a declaration, only a summons, not some concrete move contributing to the solution of the Jewish question.”⁷⁵ The peasants continued to pass over the Camp’s anti-Semitism, and *Nasz Przegląd* asked warily whether this was the same old Sanacja line, which the community had only recently rejected so convincingly, or whether, “in the face of the series of elections yet to come in some cities, [the Camp] needs an anti-Semitic pony upon which, in its view, to ride to future triumphs.” Echoing the thoughts of many nationalists, the Jews wondered why, if the OZN, with a decisive majority in the Sejm, wished to correct the government’s actions, it did not do so through legislation instead of interpellation. “Then, indeed, we would all be convinced that the Ozon has some kind of positive proposition to make.”⁷⁶

⁷³ *Gazeta Polska*, Dec. 4, 22, 1938.

⁷⁴ *WDN*, Dec. 24, 1938; see also the issue of June 1, 1939.

⁷⁵ *ABC*, Dec. 23, 1938; see also the issues of Dec. 22, 27, and 29.

⁷⁶ *Nasz Przegląd*, Dec. 23, 24, 1938. On the Nationalist stand, see *Jutro Pracy*, no. 1, 1939.

Although the Socialists did not respond directly to the interpellation, in early January they turned their attention to this newest manifestation of official anti-Semitism, which one analyst thought was an artificial movement with no real basis "other than political calculation." Niedziałkowski, far less optimistic, restated the "crisis of conscience" facing the Sanacja as "having to decide among themselves whether Roman Dmowski or Józef Piłsudski will be its 'spiritual hetman' before it finds its proper place in Poland (as a political camp)." ⁷⁷

With the advent of 1939, however, the dominant role was assumed by that group within the regime favoring the adoption of a more totalitarian political system, and the Sanacja's anti-Semitism reflected this realignment of forces. The meeting on January 23 of Skwarczyński, Wenda, the heads of the Camp's propaganda and political bureaus, and professors from the various universities, reportedly called to discuss "the Jewish matter in the institutions of higher learning," was a grim harbinger of the future for Poland's Jews.⁷⁸ Equally ominous in its implications was the premier's reply to Skwarczyński's interpellation, which pledged the government's tireless efforts to develop a viable emigration policy and exhorted the petitioners to strive for the Polonization of economic and social life through peaceful means, avoiding demagoguery and violence, until such a policy could be developed.⁷⁹ In a heated commentary on this entire exchange of official views Singer charged that the state position had at last become openly discriminatory, for it had made a political as well as an economic issue of the Jews.⁸⁰

Subsequent events proved him correct. Interpreting Składkowski's response as a sanction for a more forceful approach than had hitherto been utilized, many OZN organizations, particularly in the Pomorze region, began a campaign of concerted hate propaganda against Jewish merchants that was later broadened to include professional and civic groups as well.⁸¹ The worsening situation led the Bund to reject the conciliatory attitudes that various Polish opposition parties were adopting toward the regime, and by March Jewish optimism had reached such a low point that Singer sadly concluded that the Camp had tightened its ranks and was advancing more strongly than ever to political battle.⁸² The Jews also figured prominently in the Rada Naczelną's March deliberations. The plan for industrializing Poland resulting from that session emphasized that "an especially important joint task for the State and the community is the strengthening as well as

⁷⁷ *Robotnik*, Jan. 10, 14, 1939.

⁷⁸ As described in the military political report, "Weekly Situation Report," no. 5, 1939, CAW/DOK II, t. 143.

⁷⁹ *Gazeta Polska*, Jan. 24, 1939.

⁸⁰ *Nasz Przegląd*, Jan. 25, 1939; see also the issue of Jan. 29.

⁸¹ See the organizational circulars published by the Pomorze regional headquarters for February and March, AAN/OZN, t. 31.

⁸² The opposition activities and the Bund reaction are documented by the military reports in "Weekly Situation Report," nos. 2, 14, 15, 1939, CAW/DOK II, t. 143. For Singer's comment see *Nasz Przegląd*, Mar. 6, 1939.

the acceleration of the Polonization of all industries and trades to guarantee the Polish element a decisive role in these spheres of the country's growth."⁸³ When the Rada presented its rather irrational demand for Polish colonial expansion, lavishly supported with detailed arguments proving its necessity to the country's development, it curtly assured the Poles that "under no circumstances can we merge our needs for colonies with the possibility of emigration for the Jews. Poland requires colonies exclusively for her own Nation, and not for the Jews."⁸⁴ The final chapter in the chronicle of Polish Jewry's relations with the group that governed Poland for most of her existence between the world wars came at the end of July when, with pressure from Hitler mounting daily, *Gazeta Polska* brusquely rebuffed Jewish overtures for a reconciliation with the Polish state by announcing: "The fact that our relations with the Reich are worsening does not in the least deactivate our program in the Jewish question—there is not and cannot be any common ground between our internal Jewish problem and Poland's relations with the Hitlerite Reich."⁸⁵

SHORTLY BEFORE THE OUTBREAK of World War II the Russian philosopher Nikolai Berdyaev examined the wave of anti-Semitism then sweeping over Central and Eastern Europe. While stressing that this doctrine was contrary to the Christian ideals that so many of its practitioners professed, Berdyaev made the more pertinent observation that "the 'Jewish question' is not only sinful and inhuman, it is evidence of serious weakness and incapacity. There is something degrading about the fact that those who in fear and hatred consider the Jews so powerful are thus labeling themselves as weaklings, incapable of holding their own in free competition with the Jews."⁸⁶

This was especially true in the case of Poland. To the traditional economic, social, and cultural grounds for anti-Semitism the Polish leaders added a political motive: the desire to fashion a solid base for their regime by excluding one group of citizens from participation in the life of the state, while openly recognizing the favored position of another portion of society. In this manner Piłsudski's successors sought to form a cohesive core of political leadership around which the general mass of Poles would unite, however grudgingly, as they came to realize the growing gravity of the world situation. Yet in pursuing this policy the Sanacja displayed the overall incompetence and inexperience that it revealed whenever confronted with problems requiring statecraft for their solution. Like all the regime's pro-

⁸³ The theses on industrialization were published in *Gazeta Polska*, Mar. 7-9, 1939, and in brochure form as *Uprzemysłowanie Polski. Tezy Rady Naczelnej* (Warsaw, 1939).

⁸⁴ This portion of the theses was likewise published as *Tezy Rady Naczelnej w sprawach polskich żądań kolonialnych, planowego przygotowania sił fachowych, polityki ludnościowej i społeczno-zdrowotnej* (Warsaw, 1939).

⁸⁵ *Gazeta Polska*, July 23, 1939.

⁸⁶ "The Crime of Anti-Semitism," *Commonweal*, 29 (1939): 707.

grams in the domestic sphere, official anti-Semitism was a halfway measure, manifesting the outward forms of this strategy as practiced in Germany and Romania but lacking the essence and substance that had given those movements a major chance of success. The leaders of the country understood perfectly that the thorough, total implementation of anti-Semitism in Poland would have necessitated many violent outbursts, which in turn would have turned most Poles against the government and eventually resulted in the loss of the Sanacja's precarious hold on the state. The veteran nationalist practitioners of true anti-Semitism, however, swiftly perceived that the regime's attempts were a hollow sham. The Sanacja consequently discovered that it had opened a veritable Pandora's box, for whatever tentative steps it took were so thoroughly exposed by the Endeks, ONRists, and other nationalists as devoid of real substance that its leaders felt compelled to escalate their programs far beyond the original plans to please the recalcitrant objects of their affections. Moreover, the regime not only failed to win over the nationalist opposition to their side but, through their clumsy and ill-advised application of this policy, alienated many within the Sanacja who had been among the founders and staunch supporters of Piłsudski's movement. It is a tribute to the political maturity and cultural humanity of the vast majority of Poles that they resisted this overt appeal to baser instincts and thereby thwarted the regime's designs of dividing and ruling along lines of hatred, passion, and alienation.

Chilean Rural Labor in the Nineteenth Century

ARNOLD J. BAUER

THE DECADE OF THE 1860s marks an important stage in the history of those parts of the Western world that have since been called developed and underdeveloped. In the Great Plains of the United States, in Canada, and in Australia huge exports of bulky food to the world's industrial centers were made possible by changes in transport—better sailing ships and improved knowledge of ocean geography, rail, and steam. In these new lands where the indigenous occupants could not be induced to work, where there were no archaic farming practices to overcome, agricultural expansion brought into existence wholly new rural societies—technologically up to date from the start. Tens of thousands of new landowners appeared; farmers who had no choice if they were to survive but to use the new Australian machines or McCormick's or Deere's latest invention. The result was a vigorous agricultural sector capable of supplying a large urban population and able to consume its industrial output.¹

In other regions of the West agriculture had long been undergoing important changes, but the competition and heightened demand of the later nineteenth century made progressive adjustments imperative. In England and such parts of the Continent as Denmark and Holland, agriculture was mechanized and diversified; a reduced rural population was more intensively employed. Elsewhere in Western Europe responses varied but everywhere a more efficient agricultural base was created. The industrial cities that absorbed the surplus produce and population became increasingly politically dominant.²

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¹ A. J. Youngson, "The Opening up of New Territories," in *The Cambridge Economic History of Europe*, 6, ed. H. J. Habakkuk and M. M. Postan (Cambridge, 1965), pt. 1: 139–211; Edgars Dunsdorfs, *The Australian Wheat-Growing Economy, 1788–1948* (New York, 1956).

² B. H. Slicher Van Bath, *The Agrarian History of Western Europe, A.D. 500–1850*, tr. from the Dutch by Olive Ordish (London, 1963); Folke Dovring, "The Transformation of European Agriculture," in *Cambridge Economic History of Europe*, 6, pt. 2: 604–72; J. H. Clapham, *The Economic Development of France and Germany, 1815–1914* (4th ed.; Cambridge, 1963); Michael Tracy, "Agriculture in Western Europe: The Great Depression 1880–1900," in

For Chile and other regions, however, participation in the Atlantic economy led to change in another direction. Like other upland areas of Latin America where indigenous farmers survived the European conquest, Chile at the end of the colonial epoch was a land of large estates worked by a rural population with deeply ingrained habits.³ The agrarian structure was little altered by independence (1810–19) or by the political changes of the next three decades. Then, around 1860, the agricultural demands of the North Atlantic economy were felt in Chile with increasing strength. Grain exports to England grew while demand from the Atacama mining region—itsself an indirect result of European economic activity—began to increase. During the next few decades Chile produced its all-time high in agricultural exports. All this altered the system of rural labor, but in such a way that the already archaic agrarian structure was reinforced.

The changes in nineteenth-century Chilean rural society have been largely overlooked. Indeed the commonly accepted view—usually based on George McBride's work in the 1930s and on more recent critics who have condemned the backward, "feudal" landlords—is that the system of rural labor established in colonial times remained unchanged until a few years ago. My purpose here is to point out the impact of the nineteenth century and to explain how and why change came about. Let us begin with a description of rural Chile in the decades before the opening of the Atlantic market.

THE MOST CONSPICUOUS FEATURE of Chilean rural life in the first half of the nineteenth century was its constancy. Despite the formal end of one imperial attachment and the informal beginning of another, regardless of nearly two decades of political strife, new proclamations, and constitutions, little occurred to disturb the languor of country life. A few sacks of grain still moved by muleback or oxcart for export to Peru; the large estates lay in isolated neglect populated by droves of cattle and casual workers.

Three main groups worked on the land. We are left with imprecise information about one of these groups, proprietors who farmed their own small-holdings; the number of their "family farms" is difficult to determine from the only two sources of data available. The national censuses contain a category called *agricultores*, in which all landowners, renters, and service tenants are grouped together. The other source—the tax rolls and *catastros*—provide another indication, but they do not include the smallest pro-

Charles K. Warner, ed., *Agrarian Conditions in Modern European History* (New York, 1966), 98–111.

³ This description omits the "lowland plantation" estates that were used mainly for growing tropical crops in the Caribbean or Circum-Caribbean region, and the new, rich lands of Uruguay and the Argentine Pampa, first settled in the second half of the nineteenth century. See Eric Wolf and Sidney Mintz, "Haciendas and Plantations in Middle America and the Antilles," *Social and Economic Studies*, 6 (1957): 380–412.

prietors. But regardless of the number, the contribution of "family farms" to commercial agriculture was insignificant. Upward of eighty per cent of all land—and an even higher percentage of the best land—was contained in large estates. If one speaks of land that produced for the market, the feudal maxim *nulle terre sans seigneur* is applicable to central Chile.

Direct labor on these large properties was supplied by the two remaining—and by far the most numerous—kinds of workers: the *inquilinos*, or service tenants, and the peons (*gañanes* as they are sometimes called), or seasonal day laborers. The institution of *inquilinaje* grew out of a century-long process in which unattached non-tribute-paying men were gradually transformed into permanent residents on the large estates. Between the late seventeenth and the early nineteenth centuries agricultural land increased in value. This was accompanied by social stratification between landholders on one side and the landless "poor Spaniard"—usually mestizos—on the other. As this took place the latter's perquisites and duties as residents became more specific. The older designation of "renter," which after all could be applied to men of high rank, gradually gave way to the term *inquilino*. The new usage reflected not only a change in function but the eighteenth-century social stratification as well.⁴

By the time Claudio Gay carried out his investigation of Chilean agriculture in the 1830s *inquilinaje* was a common if still incompletely formed labor system. *Inquilinos* were settled on large estates throughout Chile but they were mainly concentrated in those areas that produced for the small export wheat market. Cultivation obviously required more resident laborers than livestock did and so there were more on the arable estates of the central valley than anywhere else. Until grain cultivation spread south, *inquilinaje* below the Maule River (see map) barely existed.⁵

Service tenancy was not, of course, limited to Chile. In Middle America and the Andean highlands, the terms *peón acasillado*, *huasipunguero*, and *yanacóna* all describe men who exchanged labor for the privilege of cultivating a tiny plot of estate land. In times of labor scarcity or where well-established indigenous communities provided an alternate means of subsistence, landowners often used the additional device of debt to bind workers more closely to the estate. This, as we shall see, was rarely needed in Chile. But everywhere in Spanish America the system was weighted heavily in favor of the landlords. They had a near monopoly of the land, they were virtually politically autonomous, and they always enjoyed a free hand in the administration of their estates.

Service tenancy developed in Europe as well and may have reached its greatest extension in the late eighteenth and in the nineteenth centuries.

⁴ Mario Cóngora, *El Origen de los "inquilinos" en Chile central* (Santiago, 1960), 13–16, discusses the origin and development of *inquilinaje* up to about 1830.

⁵ Claudio Gay, *Historia física y política de Chile: Agricultura* (Paris, 1862, 1865), 1: 193. Gay, a Frenchman, came to Chile in 1829. He wrote a multivolume work on Chilean history and the best nineteenth-century study of Chilean agriculture.

There are many terms, including *Statartorp* (Swedish), *Robota* (Czech), and *Instleute* (German), that describe this practice. Just how rural labor systems arose and the conditions under which they were modified is a complex subject; indeed, it has been suggested as one of the keys to understanding modern society.⁶ Close analogy with Europe is difficult, however, since few Latin American rural societies were put under the same kind of pressure at the same time. If Chilean *inquilinaje* in the first half of the nineteenth century seemed casual and mild in comparison, it was because it had not yet felt the stepped-up demand common in an industrializing Europe. In general, because of the lack of strong local markets and the autonomy of the landlord class, Latin American service tenancy was probably most akin to that of Eastern Europe—especially to the system of “robot” labor.⁷

Although similar to the “robot” and to labor systems in other parts of the Western Hemisphere, *inquilinaje* was a peculiar institution. From Claudio Gay and a number of other contemporary accounts, a description of the *inquilinos* in the first half of the century may be pieced together with some confidence. Unlike medieval manorial labor the *inquilinos* were not bound legally to the land nor by custom or practice to the community. They settled in loose groups on the estates. Usually their dwellings lined the hacienda access roads, but at times they were placed in the extremities of the large properties to help keep the cattle in and the thieves out. There was no village into which they fitted, no fixed farming system that required their participation—they were not bound by “submission to common agricultural practices.”⁸ Although credit or supplies may have been advanced to the *inquilinos*, central valley estates had little need to bind labor to land through debt. A hacienda with good land to let had no difficulty attracting resident labor and we may be certain that there were always men willing to accept the limited plots available on the estates. Gay’s major criticism of *inquilinaje* was the lack of suitable leases between owner and tenant. Seeing nothing “contrary to justice” or unusual in paying one’s rent in labor, Gay went so far as to suggest that many families in France “would subscribe with pleasure” to a similar arrangement.⁹

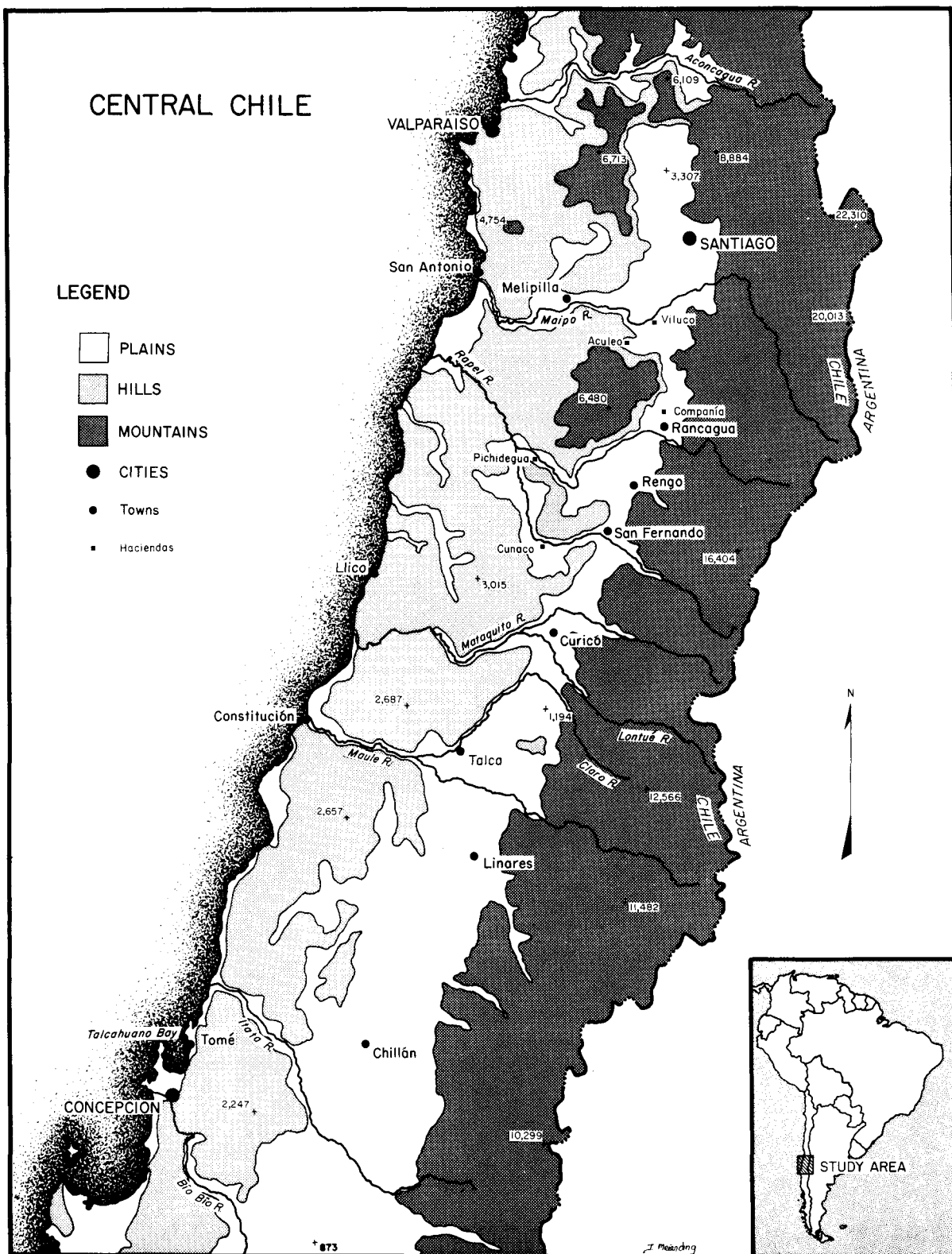
In the absence of regulation or formal contracts arrangements varied from district to district and even from one estate to the next. In the 1840s

⁶ Magnus Mörner, “A Comparative Study of Tenant Labor in Parts of Europe, Africa and Latin America 1700–1900: A Preliminary Report of a Research Project in Social History,” *Latin American Research Review*, 5 no. 2 (1970): 3–15; Barrington Moore, Jr., *Social Origins of Dictatorships and Democracy: Lord and Peasant in the Making of the Modern World* (Boston, 1967).

⁷ Jerome Blum, *Noble Landowners and Agriculture in Austria, 1815–1848* (Baltimore, 1943), 71–74, 171–87. See also Max Weber, *General Economic History* (New York, 1961), 78–81.

⁸ As was, for example, the medieval serf. See Marc Bloch, *Feudal Society*, tr. L. A. Manyon (Chicago, 1964), 1: 242.

⁹ As late as 1870, when the need for labor was much greater, *inquilinos* lined up to obtain a position on the estates. Santiago Prado, “El Inquilinaje en el departamento de Caupolicán,” *Boletín de la Sociedad Nacional de Agricultura* (hereafter *BSNA*), 2 (1871): 378; Gay, 1: 182–85.



Map: Thomas Meierding and Silvia Hernández, Department of Geography,
University of California, Berkeley.

an "average" *inquilino* received a *cerco* (garden plot) of from two to six hectares of watered land, grazing rights for ten to twenty animals, a modest shack (or the materials to build one), often a few hectares of land for sharecropping on the estate, and a daily food ration. Many observers during this period comment on the ability of *inquilinos* to accumulate wealth, and although these cases may have been exceptions it was clearly possible through good management to make money and move into the ranks of the small, or even medium-sized, landholders.¹⁰ Some *inquilinos* were "permitted up to five hundred cows . . . or have produced more than 1,000 fanegas [72 kilos] of wheat." The later descriptions that stress the upward mobility of *inquilinos* draw their examples from the early period.¹¹

Just as the *inquilinos'* perquisites varied widely so also did the amount of labor required of them. Obviously they went together. In return for the perquisites described above, the "average" *inquilino* in the first half of the nineteenth century was required as a matter of course to assist in all rodeos and in the grain and grape harvests. Besides this he worked two or three days each week for the estate in a variety of agricultural labors and odd jobs. Those *inquilinos* with access to more land or the right to graze more animals correspondingly had greater obligations. The *grado más alto* of *inquilinos* during Gay's time was required to make available to the estate one full-time laborer. Often this *peón obligado* (the man who fulfilled the *inquilino's* labor responsibility) could be drawn from among the adults who lived in his house or from among the nearby smallholders or ambulatory peons. It was the *inquilino's* strict responsibility to see that the worker presented himself for work and to remunerate him (the estate only supplied food). In this system the more favored *inquilinos* ("the better class" of *inquilinos* as foreign consular officials called them) became a kind of labor broker who recruited workers, provided their maintenance, and insured that they were on the job on time. If the *peón obligado* did not turn up, the *inquilino* was required to work in his place.

As grain cultivation spread, other forms of indirect labor came into wider use. Part of the estate land was rented for cash, and, increasingly, certain sections were let to sharecroppers (*medieros*). Livestock could also be raised *a medias* but sharecropping really dates from the increase of wheat farming and particularly the exploitation of the Coast Range. Sharecroppers were commonly drawn from among what might be called the more elevated class of the rural population: the *inquilinos*, *mayordomos* (managers), and small or medium holders. The *hacendado* (landlord), often the only source of credit, usually supplied the seed and oxen for traction while the *mediero* provided labor. In other cases the hacienda

¹⁰ *Ibid.*, 173-75.

¹¹ Atropos, "El Inquilino en Chile. Su vida. Un siglo sin variaciones, 1861-1966," *Mapocho*, 5 (1966): 214. This article, by a writer I have not been able to identify, first appeared as "El inquilino en Chile," in *Revista del Pacífico*, no. 5 (1861). Luis Correa Vergara, *Agricultura chilena* (Santiago, 1939), 2: 394-98.



Fig. 1. Photograph of central Chile, near Limache, 1860. The Oliver Collection. Courtesy of the Bancroft Library, Berkeley.



Fig. 2. Photograph of a typical hacienda in central Chile, ca. 1860. The Oliver Collection.
• Courtesy of the Bancroft Library, Berkeley.

paid half the cost of harvest labor and the rental cost of mares for threshing.¹² Estates normally had mixed systems. Part of the land was worked with resident and seasonal labor while the rest was let—usually the poorer land—to sharecroppers.

Female members of the *inquilino* household also found work on the estates. Servants, cooks, and milkmaids were usually wives or relatives of the *inquilinos*. The women were more important economically, however, in another way. Rural women produced most of the cloth and the finished garments, hats, shoes, and other items of household manufacture. Although the effect of cheap British cottons was felt by the 1840s in the northern mining districts and in the larger cities, most of the rural area of central Chile still produced its own clothing. "The Chilean *campesino*," Gay noted, "isolated in the countryside and far removed from society sees the necessity of being at once his own weaver, tailor, carpenter, mason, etc." Urizar Garfias was struck by the extent of household industry in Maule, where, although there were no formally established textile industries there was "considerable activity in making *bayeta* [a coarse woolen cloth], wool socks, *mantas*, and *ponchos*." On a trip to the province of Talca in 1852 Gilliss found that "a large proportion of the ponchos, blankets, church carpets and rugs and coarse cloth are . . . of domestic manufacture, showing that the poorer classes of women are not idle beside their spinning wheels and hand-looms." The Talca newspaper in a rare statistic estimated that 36,000 yards of cloth, 12,000 ponchos, and a variety of boots and shoes were made annually.¹³

There is a good description of household industry and a hint of the coming change in Gay:

The women [of the *inquilinos*] occupy themselves while waiting to prepare meals by spinning wool that they themselves have previously dyed yellow, blue, red, and green. Their dresses consisted before of a kind of loose wool spun and woven by these same women and dyed most always blue with the indigo obtained from Central America.¹⁴ Today, they prefer to sell the wool and cover themselves with the cotton cloth the foreigners—and above all the English—bring in at low prices.¹⁵

The beginning of the change from homespun to imports that Gay noticed was very likely the difference between what he observed during his first stay in Chile from 1829 to 1842 and the information he later received in Paris from Chileans on tour.¹⁶ During the interval increased British trade

¹² Gay, 1: 115–21; *BSNA*, 2: 381; *El mensajero de la agricultura*, 2 (1856–57): 204–06; *BSNA*, 2: 384; Archivo Nacional, Santiago, Colección Judicial (Talca), Leg. 359, p. 2.

¹³ Gay, 1: 159; Fernando Urizar Garfias, *Estadística de la República de Chile: provincia de Maule* (Santiago, 1845), 92–94; Gilliss, *U.S. Naval Astronomical Expedition to the Southern Hemisphere* . . . (Washington, 1855), 1: 57; *El Alfa* (Talca), Jan. 10, 1849.

¹⁴ The inventory of the Cunaco hacienda store shows seventy-five pounds of indigo (*añil*) at 1.20 pesos a pound. Account book of the hacienda of Cunaco, in the possession Sr. Manuel Valdés Valdés, Santiago.

¹⁵ Gay, 1: 163.

¹⁶ Gay's investigations were carried out from 1829 to 1842, but the first volume of *Agricultura*

permitted country people to substitute cooler material for coarse and scratchy woolens; but at the same time the flood of Lancashire cloth drained the country of specie and began to wipe out the spinners and weavers (*hilanderas* and *tejedoras*), an important segment of the household industry. The effect of cheaper imports was not felt, however, by those who made garments from this cloth or other manufactured items. Rural people continued to make their own clothing, sandals, hats, and ponchos with little change throughout the nineteenth century.¹⁷

The bare data and scant sources we have seen reveal how little is really known about the rural worker in the nineteenth century. There are no diaries written by *inquilinos*, no "confessions" or memoirs. The few travelers who undertook to describe rural life rarely strayed from the beaten track through central Chile and confined their comments to those model haciendas where the generosity and charm of the owner undoubtedly helped dull the edge of the visitor's critical faculties.¹⁸ Nor do the public documents provide much light. The contracts between owner and laborer were verbal, the arrangements informally agreed upon as new owners or lessees took possession of the estate. The rural worker's complaints, or even his crimes, were usually too insignificant to be recorded in the provincial court. The countless pages of litigation useful for information on the lower classes in the colonial epoch are here in the republic nearly nonexistent. The landowner himself—or his administrator—summarily dispensed justice, distributed his workers' land in the absence of surviving heirs, and settled most accounts without recourse to the troublesome and lengthy process that was common in the lawyer-filled upper levels of society.¹⁹ Even in the private accounting records of haciendas the *inquilinos* are inconspicuous. Occasional entries tell of rations for *inquilinos* and others note an advance of a *fanega* of corn or merchandise against his account.²⁰

We are left with little information about *inquilinaje* before 1850 partly

was not published until 1862 and the second was not published until 1865. For background to Gay's work see *Correspondencia de Claudio Gay*, compiled with preface and notes by Guillermo Feliú Cruz and Carlos Stuardo Ortiz; tr. Luis Villablanca (Santiago, 1962).

¹⁷ The overall data on professions before 1865 are difficult to deal with. Between then and 1895 the spinners and weavers continued to disappear (from 18,000 in 1865 to 4,000 in 1895 in the Maipo-Maule Zone). See the national censuses of 1865 and 1895.

¹⁸ There are many travel books that deal with nineteenth-century Chile, but only a few have worthwhile information on rural life. See Guillermo Feliú Cruz, *Notas para una bibliografía sobre viajeros relativos a Chile* (Santiago, 1965).

¹⁹ All civil cases involving less than 12 pesos were handled by the political head of the *distrito*, the *inspector*. His decision could not be appealed. For cases of 12 to 40 pesos his decision could be appealed to the *subdelegado*, and cases from 40 to 150 pesos were handled by the *subdelegado*. Effectively, only cases concerning medium or large landowners ever reached the departmental *jefe de letras* (and therefore are available in the judicial collection). The Archivo de Intendencias, which has not yet been organized, may contain information on the lower rural society. For an idea of how provincial justice was supposed to have been dispensed, see *Manual o instrucción para los subdelegados y inspectores en Chile* (Santiago, 1860). The system described was reorganized in the 1870s.

²⁰ This is how *inquilinos* were remunerated on the haciendas of Cunaco and Pichidegua. Account book of the hacienda of Pichidegua (hereafter Pichidegua), in the possession of Sergio de Toro, Santiago.

because the system worked and imposed no excessive burdens. Later in the century, when the economy required greater output, the rural labor system was examined and discussed. Before that time the *inquilinos* lived and worked virtually unnoticed. On the great haciendas of central Chile where good land was abundant *inquilinos* formed a stable, permanent labor force. An eyewitness of the 1850s noted that

there is not a hacienda that does not have its ancient families of *inquilinos* . . . and just as Santiago has its great landowning families of Larraín, Errázuriz, Viña, Cerda, and Toro; Talca its Cruces, Vergaras, and Donosos . . . so also each hacienda has its notable families of Ponces, Carranzas, Carocas, Aquilas, Montesinos, Pobletes, etc.²¹

The hacienda was the *inquilino's* only *patria*.

The *huaso* [*inquilino*] knows there are Englishmen, Frenchmen, and Spaniards because now and then he has seen people from these countries or has heard them mentioned at least. But he has no idea where England or France or Spain might be. He has heard of *godos* and *patriotas* and knows that both made war but who they were or why they fought he has not the slightest idea or interest. . . . The *inquilino* believes himself to be indigenous to his hacienda . . . and if he were transported to Paris or London and interrogated there as to the country of his birth . . . he would not answer "Chile," but "Peldhue," "Chacabuco," "Huechún," or "Chocalán." He has no idea if his ancestors are Spaniards, Englishmen, Russians, or Chinese. . . . He has heard people talk of Spaniards and Indians, but he does not imagine that he has had contact with those races or that their blood circulates through his veins.²²

This description by an anonymous contemporary can probably be accepted for a great many large estates in the early nineteenth century. Most writers agree that conditions were different on newer or smaller haciendas. This is suggested but rarely observed, since most visitors from Santiago or abroad passed their time at the traditional estates easily accessible from the capital. Most of Gay's description, for example, is based on the great hacienda of La Compañía, a favorite of many travelers. Darwin was an exception to most foreign visitors. His scientific interests led him into more out of the way places, which perhaps explains why he found the *inquilinos'* situation to be worse than others did.²³

Whether the smaller haciendas actually exacted more from labor or whether the comments reflect a prevailing prejudice in favor of the rural oligarchy is difficult to say. Gay thought the *inquilino* suffered when he "fell into the hands of a 'pequeño hacendado' " or "one of those 'hacendados ávaros' who do not hesitate to take advantage of their position to exploit

²¹ Atropos, "El Inquilino en Chile," 200.

²² *Ibid.*, 200-01.

²³ Gay, 1: chs. 8, 9, 10; Ramón Domínguez, *Nuestro sistema de inquilinaje* (Santiago, 1867), 33-64; John Miers, "La agricultura en Chile en 1825," *Mensajero*, 2 (1856): 127, for a description of Ocoa; Charles Darwin, *The Voyage of the Beagle* (Natural History Library ed.; Garden City, 1962), 339.

and at times even to oppress them." Horace Rumbold, British consul in Santiago, noted the same difference: "[The *inquilino*] certainly fares better in every way on the broad domains of the Correa or Larraín families or on the Vicuña estates . . . than he does on many smaller properties that shall be nameless."²⁴ Why this should have been so is not clear. Was a new group of *hacendados*—"capitalist-proprietor empresarios," as has been suggested—responding to new markets and pushing labor harder than the more traditional owners?²⁵

Whatever differences there may have been among the estates we are still left with the impression that *inquilinaje* was—at least compared with what followed—not oppressive, that it was still an easy going labor system. If inefficient it nevertheless provided suitable workers in a society where obedience and loyalty were valued over productivity. Numerically few, the *inquilinos* were the cream of rural labor. "Since the *hacendado* is concerned that the people on his hacienda be honorable . . . the body of *inquilinos* is always composed of the healthiest part of the lower class [*bajo pueblo*] that lives in the country."²⁶ This selectivity was made possible by the limited need for estate labor and the lack of alternatives open to the numerous rural families. The good fortune of being accepted on the hacienda was repaid by the *inquilinos* with service and loyalty.

LESS FORTUNATE IN THE FIRST HALF of the century were the peons—"a miserable and vagrant sort"—who made up the other major segment of rural labor. In the period 1700–1835 population grew rapidly throughout most of Europe and America. Although Chile shared with more economically advanced countries much of the knowledge and improvement that made demographic growth possible, unlike other colonies she lacked the means to employ the increased numbers.²⁷

Only a fraction of the population could be absorbed permanently on the estates as tenants while the system of latifundia made it practically impossible for newcomers to acquire private property. Some men sought work in the northern mines, a few signed up for construction work on the Copiapó rail line, and others helped build the new cart roads and bridges in the 1840s and 1850s. The great majority, however, formed a loose, unattached mass of people who squatted on marginal land along the coast or on the edge of cities, lived in rude huts on interstitial plots in the valley, or simply moved along the length of central Chile in search of

²⁴ Gay, 1: 185; Horace Rumbold, *Reports by Her Majesty's Secretaries . . . on the Manufactures, Commerce, . . .* (London, 1876), 396.

²⁵ Silvia Hernández, "Transformaciones tecnológicas en la agricultura de Chile central. Siglo XIX," *Cuadernos del centro de estudios socioeconómicos*, no. 3 (1966): 14–16.

²⁶ Atropos, "El Inquilino en Chile," 206.

²⁷ *El Agricultor*, no. 21 (Feb. 1842): 159–68; Marcello Carmagnani, "Colonial Latin American Demography: Growth of Chilean Population, 1700–1830," *Journal of Social History*, 1 (1967): 179–91.

sustenance. The benign climate and fertility of the land made it possible for them to exist; lack of economic activity gave them no alternative. Most of them could work but few would do so for the remuneration offered. Under the circumstances many preferred to live off the land and supplement their foraging with petty theft.²⁸

Rural settlement in central Chile contrasted with that of countries that had a more advanced indigenous population or, for that matter, with the urban tradition of Mediterranean Europe. Before 1850 there were few villages and hardly any rudimentary settlements. Except for the agglomerations of residents on the haciendas and a handful of provincial towns, the population was widely scattered. Gay wrote about the rural Chileans that "their love of isolation . . . so opposed to the spirit of Latin race, which is always quick to group its dwellings in small hamlets [was proof that] European blood had mixed very little in this class of society." The Crown had made repeated efforts in the eighteenth century to establish a few towns as provincial capitals, but by 1850 (except for Talca) even these consisted of little more than a single long dusty street.²⁹

Occasional attempts were made to encourage the floating population to settle in hamlets, but there was too little incentive for the peon or too little control by the authorities to accomplish this. The peon undoubtedly understood that concentration would threaten his semilegal means of support, and given the choice of organized or free poverty he chose the latter. In 1839 a Franciscan friar wrote that in the "vast and wide fields [near Rosario on the coast] there are more than 10,000 souls who have no place to pitch their 'tristes ranchos.'" He proposed to settle them on Church lands and enlist the cooperation of local *hacendados*.³⁰ The plan failed but the proposal suggests the magnitude of the problem. Many documents of the 1840s and 1850s refer to the floating population. A proposal to develop the port of Llico, for example, suggests that work crews could be formed out of the vagrant population ("de los muchos vagos que hai en el puerto"). The intendancy archive of Talca contains countless references to theft and vagrancy that resulted from "the large number of evildoers" in the prov-

²⁸ An example of these labor contracts is the one drawn up by William Wheelwright for 119 peons for six months at three *reales* a day plus rations. This was for the Copiapó railroad in 1850. Archivo Nacional, Santiago, Colección Notarial (Valparaíso) vol. 86, fol. 355, vol. 88, fol. 94. See also *El Mercurio* (Valparaíso), no. 7606, Jan. 7, 1853, for a note on "gran caravana de hombres enganchados para las minas de Copiapó y el Huasco." For a discussion on vagabondage and floating population for an earlier period, see Mario Góngora, "Vagabundaje y sociedad fronteriza en Chile (siglos xvii a xix)," *Cuadernos del centro de estudios socio-económicos*, no. 2 (1966): 1-41.

²⁹ Gay, 1: 155; Archivo Nacional, Santiago, Archivo del Ministerio de Hacienda, vol. 362 (1859), no fol., has reports by the administrator of the Estanco (State tobacco monopoly) to the intendant: "The population is very scattered; there are a very few small 'aldeas' [hamlets]." This picture is borne out by censuses. The Estanco administrators often provide worthwhile information about business conditions in the provinces.

³⁰ Gay, 1: 155; *El mensajero de la agricultura*, 2 (1856): 270-76. The friar was Padre J. J. Guzmán, whose candid description of rural Chile, the *Mensajero* reminded its readers, "was that of a worthy priest, not a 'socialista o republicano rojo.'"



Fig. 3. A hacienda store. Gathering place for rural workers. Reprint of a lithograph from Claudio Gay, *Atlas de la historia física y política de Chile* (Paris, 1866), vol. 1, pl. 12.



Fig. 4. Dwelling of a nineteenth-century rural worker in central Chile. The Oliver Collection. Courtesy of the Bancroft Library, Berkeley.

ince. Moreover, the mobility of the peons is impressive. They moved from one work gang to another several miles away for an additional six centavos or an "extra bean in the pot."³¹

For the less arduous labor on the haciendas peons were attracted for little pay. For tasks such as harvest or roundup when outside labor was required, quantities of local wine (or *chicha*, a fermented grape drink) and food and the promise of frolic were sufficient inducements for the floating laborer. The name for this form of "fiesta labor" is a word of Quéchuá origin: the *mingaco*. It may be defined as a "fiesta or reunion celebrated to do something in common, a work that requires the participation of many people. . . . The characteristics of a *mingaco* are that nothing is paid except . . . a *comilona* [gluttonous meal] accompanied by drink, song, and dance."³² In 1840 the Society of Agriculture circulated a questionnaire to several landowners asking, among other things, "Which are more advantageous: the *mingacos* or daily wage earner?" Unfortunately, few responses were recorded. One respondent from near Rancagua replied that his *inquilinos* were adequate for the task, but if they had not been he would be disinclined to use the *mingaco* "because with them it is necessary to tolerate drunkenness." The tone of the article suggested that the *mingaco* was a common labor form. And Gay notes that "the expense in money is almost nothing when the use of the 'mingaco' is adopted. . . . Workers are always ready to lend their services to this kind of fiesta."³³

That seasonal labor could be attracted through the *mingaco* reveals at once the low labor requirement, the casual nature of agriculture, and the precariousness of a large body of the rural population. If this was the peons' income at harvest time it is not difficult to imagine the extent of their resources in the off-season or to understand why the *inquilinos* must have considered themselves fortunate.

Most Chilean writers and foreign visitors made a clear distinction between the *inquilino* and the peon. Julio Menandier, editor of the National Society of Agriculture's *Boletín*, and a man who knew the Chilean countryside as well as anyone in the nineteenth century, provides a typical description:

[The peons] have no real needs. To them it is the same whether they have a roof over their heads or live in the open air, whether they have good and abundant food or bad and scarce [meals]. The majority of them do not seek work unless

³¹ Archivo Nacional, Santiago, Archivo del Ministerio de Hacienda, vol. 250 (1853), no fol.; Archivo de la Intendencia de Talca, vols. 12, 15, 18. This is the only *intendencia* archive of the central agricultural provinces that is even partially organized. In 1967-68 a group of students cataloged the Archivo de la Intendencia de Concepción. Archivo Nacional, Santiago, Archivo del Ministerio del Interior, vol. 163 (1842), no. fol.

³² Manuel Román, *Diccionario de Chilenismos* (Santiago, 1913), 3: 508-09. For a discussion of the present-day practice of *mingaco* (in the Araucanía), see Charles Erasmus, "Reciprocal Labor: A Study of Its Occurrence and Disappearance among Farming People in Latin America" (Ph.D. dissertation, University of California, Berkeley, 1955).

³³ *El Agricultor*, no. 10 (Apr. 1840): 1-16; Gay, 2:34.



Fig. 5. Idle hands in central Chile. A part of the floating population near Santiago. Reprint of a lithograph from Gay, *Atlas*, vol. 1, pl. 32.

they are driven by hunger. The income from a whole week's arduous labor is lost with indifference in a single night of gambling and disorder.³⁴

Gay drew a sharp line between the *inquilino* and the peon.

The major portion [of peons] lead an entirely nomadic life . . . have no notion of order . . . and are incapable of appreciating the value of time. The rural peon, generally called "forastero" [outsider] is more thieving than the city worker. . . . [Rural peons] move from field to field looking for work and often they descend on the orchards like the plague stripping the trees to satisfy their craving for fruit.³⁵

In the off-season the peons were even less industrious, and few observers fail to comment on their "sloth." "A pernicious and vituperable lassitude—which they themselves call apathy—dominates the inhabitants."³⁶

A peak in the quantity of the floating population was reached in the

³⁴ *BSNA*, 1 (1869–70): 381.

³⁵ Gay, 1:198–203.

³⁶ F. Urizar Garfias, *Repertorio chileno* (Santiago, 1835), 7.

late 1850s. Writing of the fertile province of Colchagua in 1858, a prominent Chilean had this to say:

Open your eyes and you see daily entire families abandoning their homes and setting out—to where? Even they do not know. Their only purpose is to leave a place that does not provide a living . . . many head toward Santiago. The refrain, “me voi pa’ la ciudad” is very common among the poorer people. Travel our roads and you will see many families moving with their equipment on their backs toward the capital to augment the already present pauperism . . . even now the “vagos” cannot be controlled as they should be.³⁷

Central Chile, therefore, was an area with plentiful if idle hands. As late as 1865, out of a total population of 433,000, including 101,000 men between the ages of fifteen and fifty, nearly sixty per cent (59,000) were considered by the census takers to be “persons without residence or fixed destiny who prefer manual labor in any kind of rural or urban task.”³⁸

FROM ABOUT 1860 ON, rail and steam put European markets in reach of the peripheral zones. Chilean exports to England grew steadily while at the same time the northern mining districts and Santiago increased their demand for food. In North America, Australia, and Argentina, where no sedentary population existed, men arrived with machines in tow to produce grain for the cities of industrial Europe. Unlike these unpopulated new lands, however, central Chile contained a great many men. Whereas production in the new lands required machines, here the older farming system could be extended by inducing the existing population to work. Coincident with the expansion of cereal cultivation the labor systems began to change. Landowners demanded more service from the *inquilinos* and attempts were made to convert the peon into a more stable and reliable work force. Where independence and new governments scarcely had been noticed, the consequences of economic expansion were felt in the most remote corner of central Chile. (For an indication of population density in three regions that competed in the nineteenth-century international grain market, see table 1.)

The system of *inquilinaje* was modified in a number of ways. First, those tenants who from earlier times had been allotted extensive perquisites were required to supply the estate with additional service. Whereas one *peón obligado* was the maximum service required of an *inquilino* before 1850, now he was asked to provide two and even three full-time workers. Already in 1862, for example, as the hacienda of Pichidegua turned to wheat cultivation, the number of *peones obligados* required of each *inquilino* was stepped up. Of the twenty-one *inquilino* households,

³⁷ Daniel Barros Grez, *Proyecto de división de la provincia de Colchagua* (Santiago, 1858), 4.

³⁸ The data apply to the area between the Maipo and Maule in the map (p. 1063). See 1865 census. The definition is that used by the nineteenth-century census takers. See *Sexto censo jeneral de la República* (Santiago, 1885), 1: xiv–xv.

TABLE 1. Ratios of Population to Land Area:
Selected Areas, 1875-82

<i>Area and Population Density</i>	<i>Central Chile^a</i>	<i>Victoria (Australia)^b</i>	<i>San Joaquin Valley (California)^c</i>
Total area (Km ²)	33,500	34,000	50,500
Wheat area (Km ²)	1,500	3,656	3,628
Population	450,000	117,000	80,800
Pop. per Km ² -area	14	3.4	1.6
Pop. per Km ² -wheat	300	32	23

Sources:

^a Data are for the present-day provinces of O'Higgins, Colchagua, Curicó, and Talca—the area between the Maipo and Maule Rivers. Population data are from *Censo de 1875* (Valparaíso, 1876); wheat harvest and acreage data are from *Anuario estadístico* (Santiago, 1874).

^b Data on area under wheat from twenty wheat-growing shires in Victoria are for years 1881-82. See Edgars Dunsdorfs, *The Australian Wheat-Growing Economy 1788-1948* (New York, 1956), 489, 532-34. Population data are from the same source.

^c California data for area under wheat represents the counties of Calaveras, Fresno, Mariposa, Merced, San Joaquin, Stanislaus, Tulare, and Tuolumne. See E. E. Martin, "The Development of Wheat Culture in the San Joaquin Valley 1846-1900" (master's thesis, University of California, Berkeley, 1924), xi.

twelve had to supply two peons each. By 1870 this process had become commonplace. Menandier noted that "the work load imposed on the *inquilino* has increased many times over." In Caupolicán the "better class" of *inquilino* (or the *inquilino de a caballo*) supplied two year-round laborers each. Even the *inquilino de a pie*, who enjoyed fewer perquisites, was expected to work himself and furnish as well any son old enough to "swing a hoe." Adults living in his household (*allegados*) were also required to work. One can see in the upland hacienda of Peumo a clear picture of the relationship between perquisites and service requirements as well as the complex and varied arrangements that had evolved. The "first class" *inquilino* furnished two *peones* for the entire year. The *inquilino* was responsible for remunerating the peons; the hacienda only provided the daily food ration. It was calculated that each worker cost the *inquilino* fifty to sixty pesos a year (about twenty centavos a day). Since the peon was often a member of the *inquilino's* household, the "wage" was probably most often paid in kind. Even if the *inquilino* had to go outside his own kin for the peon, he must rarely have paid in cash. The *inquilino* was expected to remunerate his workers from his own earnings on the hacienda.³⁹

At the same time that the labor requirements of the original *inquilinos* were stepped up, new tenants settled and received reduced land allotments. This extension of *inquilinaje* in a modified form was particularly noticeable in the 1870s and 1880s. A well-known manual of operating

³⁹ Pichidegua, (1862-64), n. p.; *BSNA*, 1: 380; 2: 387; C. G. U., "Los inquilinos de 'el Peumo,'" *BSNA*, 6 (1874-75): 306-08. See also Manuel José Balmaceda, *Manual del hacendado Chileno* (Santiago, 1875), 127-28.

instructions for rural estates speaks of the *inquilino-peón* who was allotted a dwelling and a small piece of land "not exceeding forty *varas* [about thirty-five yards] to a side in order to raise hens and vegetables when there is water."⁴⁰

These new tenants were selected from among the families of the *inquilinos* or from the *peón-gañán* class. Many peons preferred to deal directly with the estate rather than work through the *inquilinos*. The tenant-peons then received a smaller *cercos* than had *inquilinos* before 1850, and if necessary this was supplemented by a small salary.⁴¹ This process was at work in 1871 on the hacienda of Viluco just south of Santiago where more than 200 *peones sedentarios* each received a house surrounded by a half-*cuadra* ($\frac{3}{4}$ hectare) garden plot. Menandier repeatedly urged the extension of *inquilinaje* and thought that the sole defects of the new dwellings on Viluco were their unnecessary comfort and high cost; it would have been better to build more and cheaper buildings to house all the hacienda peons. Furthermore, "in order to procure a good number of able, moral, and constant workers" it was necessary to increase the number of dwellings of the sedentary peons.⁴²

The 1870s was a time of economic leveling off of lower rural society. More prosperous tenants were required to contribute labor service commensurate with their perquisites, while the landless peon was raised to the only slightly higher level of tenant-peon in return for more reliable labor. This tendency was not lost on contemporary observers. An article in the *Boletín* noted that whereas in earlier times the *inquilino* as head of the household had also been the *patrón* of several *cuadras* of land, the expanded cereal cultivation had effectively led to diminished perquisites.⁴³ A comment on the "growing poverty" of the *inquilinos* that is probably only slightly exaggerated will serve to sum up the process taking place in the 1870s:

[In earlier years] the horses, cows, and sheep that often were numerous formed for the *inquilinos* a relative fortune. Does any of this exist after sixty years of independence and free commerce? . . . Unfortunately everything has been changed . . . the proprietor is obliged to obtain all the possible advantage out of the worker who now lives on his land.⁴⁴

TIGHTENING THE SCREWS ON THE SERVICE TENANTS and settling additional families on the estates was one part of the adjustment imposed on Chilean

⁴⁰ Balmaceda, *Manual*, 127-28. M. J. Balmaceda, a senator and a great landowner in the latter nineteenth century, was the father of J. M. Balmaceda, president of Chile (1886-91).

⁴¹ Félix Echeverría, "Las máquinas y el trabajador agrícola," *BSNA*, 2: 376.

⁴² *BSNA*, 3 (1871-72): 183-84.

⁴³ *BSNA*, 2: 376.

⁴⁴ Speech by Félix Vicuña, in *Sesiones de la cámara de senadores* (ordinarias), July 31, 1871, p. 62.



Fig. 6. Threshing grain in central Chile. Reprint of a lithograph from Gay, *Atlas*, vol. 1, pl. 19.

rural society in the last half of the nineteenth century. The second indispensable requirement for greater agricultural output was abundant seasonal workers (peons or *gañanes*) during the harvest season from December to late March. In neighboring Argentina—the only other Latin American country to export grain—landowners began to fill this same need by attracting thousands of seasonal workers from Italy and Spain. Still they could command fairly high wages—perhaps eight to ten times the going rate in Chile—and machines increasingly came into use on the Pampa. By 1894 nine thousand reapers and 1,500 threshers were imported annually.⁴⁵ Chile, however, had a source of cheap labor and consequently an alternative to mechanization; but this required that the large floating population become a more stable and reliable work force.

The need for seasonal labor may be best seen in reaping. Between 1850 and 1875 Chilean grain cultivation increased from about 130,000 to 400,000 hectares. In the common measurements of the time this meant an increase

⁴⁵ James R. Scobie, *Revolution on the Pampas: A Social History of Argentine Wheat, 1860–1910* (Austin, 1964), 60–61, 80–84, gives an average monthly wage of around £10 or roughly 60–70 Chilean pesos. The scale of agricultural output in the late nineteenth and early twentieth centuries in Argentina far surpassed that of Chile.

of about 173,000 *cuadras* (a *cuadra* was the equivalent of about four acres) or 1,730,000 *tareas*. The maximum output per worker was one *tarea* a day. In any one department there was a period of two to three weeks during which grain must be cut. If harvested too green, the kernel shriveled; if too ripe the grain shattered and was lost. But over the entire length of central Chile—because of different growing seasons—this period was extended to about forty-five days. Reaping began in Aconcagua in early December and continued until late January south of the Maule. Given this fact and the knowledge that some workers followed the harvest from north to south, one may assume that each worker could have cut thirty to fifty *tareas* a season. At this rate all of central Chile would have required between 35,000 and 50,000 additional workers for reaping grain.

No doubt rural labor was exceedingly inefficient. Over one hundred feast days and holidays were celebrated a year; absenteeism was high; and alcoholism, always a problem, became worse in the late nineteenth century.⁴⁶ At times peons were paid by piece-work, or in the case of reaping, by the *tarea*. A man ordinarily should cut one *tarea* a day, that is, about 1,800 to 2,000 m² (the standard U.S. rate was 2,000 m² a day).⁴⁷ Yet the fact that the wage per *tarea* was often three or four times higher than the daily rate suggests that no more than 500 m² were normally cut in a day.⁴⁸

Another indication of the massive use of labor in Chilean agriculture is seen in table 2. The data show the number of man-days used to produce thirty metric quintals (about 110 bushels) on one *cuadra* of land. Chilean labor use was at least three times as high as in the United States for a given output, the explanation for the difference being that in the United States land was more productive, technology was better (even for hand tools), capital investment was greater, and North American labor was more efficient.

As grain cultivation spread, the need for seasonal labor in Chile grew. Only when it was strictly necessary or when year-round tasks could be found to occupy them were peons settled on the estates. Resident labor occupied land and required administration and often rations. Far more preferable were workers who were readily available for the two or three months of the grain harvest and who could be dismissed when the need for them was past.⁴⁹ The difficulty lay in keeping unattached men in the

⁴⁶ *BSNA*, 12 (1880–81): 391; see also 19 (1887–88): 632. Alcoholism is invariably mentioned by travelers as well.

⁴⁷ Leo Rogin, *The Introduction of Farm Machinery in its Relation to the Productivity of Labor in the Agriculture of the United States during the Nineteenth Century* (Berkeley, 1931), 125.

⁴⁸ Newspapers advertised for workers at as much as one peso per *tarea* in 1859. *El Ferrocarril* (Santiago), Jan. 15, 1859. The accounts of a *fundo* in Linares show a daily wage of 20 centavos while 80 centavos was paid per *tarea*. Archivo Nacional, Santiago, Colección Judicial (Linares), Leg. 88, p. 13. See also Gay, 2: 33–35.

⁴⁹ The entire harvest from reaping through the *avienta* (pitching straw into the air to separate the grain from the chaff), to sacking could last until March. On some estates the *vendimia* (grape harvest) immediately followed wheat.

TABLE 2. Labor Use in Wheat Production:
United States and Chile, 1829-95

Year	Method	United States	Chile
1829-30 ^a	All hand labor	32 man days	—
1856 ^b	All hand labor	—	97 man days
1873 ^c	Machine threshing	—	53
1880 ^d	All machine	15	—
1894 ^e	Machine (Kansas)	11	—
1895 ^f	Machine (California)	1.3	—

Sources:

^a Leo Rogin, *The Introduction of Farm Machinery in its Relation to the Productivity of Labor in the Agriculture of the United States during the Nineteenth Century* (Berkeley, 1931), 72-78.

^b Claudio Gay, *Historia física y política de Chile: Agricultura* (Paris, 1862, 1865), 2: 44. I have added eight man-days for threshing and adjusted for different yields.

^c *Boletín de la Sociedad Nacional de Agricultura*, 5 (1873-74): 9.

^d *The Statistical History of the United States from Colonial Times to the Present* (Stamford, 1965), 281.

^e Rogin, 218 (unit D).

^f Rogin, 217 (unit 26). For corroboration of Rogin's data, see W. P. Rutter, *Wheat Growing in Canada, The United States, and the Argentine* (London, 1911), 110.

agricultural zone so that when the need arose it would not be necessary to increase wages to attract a sufficient number of workers.

The supply elasticity of labor is difficult to determine. We have little information on salaries let alone an index of real wages. There are the normal complications: peons were remunerated with money, payment in kind, perquisites, tokens for exchange in the *pulperia* (hacienda store), and combinations of all these. They were paid by the day, week, month, or for piece-work. Information on prices is incomplete, too. I have assembled the prices for four main items in the rural diet, but systematic studies of other food categories or of manufactures have not been carried out.⁵⁰ We know that clothing, shoes, hats, and the like were still largely handmade—whereas most cloth was imported—but we do not know their prices.

The information that is available suggests that real wages stayed constant and may have decreased slightly toward the 1890s. The best source for wages is hacienda account books, but lamentably few of these exist either from private or ecclesiastical estates. Scientific farming and the best rural accountants were both done away with when the meticulously managed Jesuit estates were sold in the late eighteenth century. A few accounting records exist in private archives, and others can be found in the judicial archival collections that pertain to lawsuits that forced owners or administrators to “render accounts.” These records, however, are fragmentary and largely unsatisfactory.

On the other hand, few observers fail to comment on peon wages. Many

⁵⁰ Arnold J. Bauer, “Expansión económica en una sociedad tradicional: Chile central en el siglo XIX,” *Historia* [Catholic University of Chile], no. 9 (1970): 223.

of these informants were casual travelers but a few—Gay, Gillis, and Rumbold among them—were knowledgeable and critical. A number of painstaking statistical surveys of various years, Urizar Garfias, *Estadística de Maule*, for example, and the numerous studies in the 1874 *Anuario estadístico*, have much information on peon wages. Finally, the SNA *Boletín*, especially in the 1870s when *hacendados* and politicians were agitated by problems of rural labor, provides several unsentimental and perceptive reports. On the basis of these admittedly scattered data, table 3 has been constructed.

TABLE 3. Average Peon Wages by Decades, 1830–80
(current pesos, centavos per day)

1830–50	15–20
1851–60	25
1861–70	25
1871–80	30

During this same period the cost of living for rural peons remained about constant. Flour prices stayed about the same (in current pesos, that is; the constant money prices decreased regularly) until the late 1880s despite the decreasing value of the peso in terms of gold, while the cost of beans, the other major staple of the rural diet, tended to increase slightly in the period 1850–90. Beef prices rose, too, but meat was an insignificant part of the peon's diet. Since prices rose later in the 1890s and because wages tend to lag, real wages most likely were falling toward the end of the century.⁵¹

The picture that is presented of a growing demand for labor along with nearly constant or falling wages—indicating an elastic labor supply—conflicts with the contemporary complaint of labor scarcity. There is hardly a volume of the SNA's *Boletín* in the nineteenth century that does not lament the *escasez de brazos* (shortage of hands).⁵² This apparent contradiction can be explained by understanding what the *hacendado* meant by labor scarcity. We must recall that labor had always been abundant. Accustomed to attracting easily a mass of unskilled labor with little if any cash outlay, the *hacendado* looked upon any temporary bottleneck that restricted his supply of labor and forced him to pay a money wage as *escasez de brazos*. Earlier in the century men could be attracted for harvests and roundups merely through the offer of food and drink. Wages paid in money were infrequent through most of the nineteenth century.

⁵¹ This is the impression gained from contemporary accounts. The *BSNA*, 13 (1881–82): 391, for example, claimed that inflation plus lagging wages meant that “all who live on salary are on half pay.” Private records become more plentiful after about 1890 and it may be possible to construct a wage series for agricultural workers from then to the present.

⁵² For a summary of the literature, see Gonzalo Izquierdo, *Un estudio de las ideologías chilenas: la sociedad de agricultura en el siglo XIX* (Santiago, 1968), 133–58.

Landowners opposed money wages—or rather justified their scarcity—with the standard remark that wages caused vice and the higher the wage the greater the vice.⁵³

From the 1850s on, as railroad and public works projects began to compete for peon labor, it is true that occasional local shortage undoubtedly occurred. But these isolated disturbances should not be misconstrued; an abundance of hands were available to Chilean agriculture throughout the nineteenth century.⁵⁴

There was in fact a surplus of men. After 1850 large numbers of people migrated from the agricultural zone. From 1865 to 1895 the population of central Chile increased from 433,000 to 475,000, a rate of only 0.3 per cent a year. But in the urban departments of Valparaíso and Santiago and the northern mining districts the increase was from 469,000 to 828,000, a rate of 1.9 per cent. The first large-scale attraction for peon labor was the Valparaíso to Santiago railroad. Begun in 1852 and completed in 1863, construction through the rugged coast range required a work force of about 10,000 men a year.⁵⁵ In 1868 Henry Meiggs, the famous "Yankee Pizarro," turned his talents to railroad construction in Peru and for the next four years migration from central Chile occurred on an unprecedented scale. Upward of 25,000 peons went to Peru between 1868 and 1872. This movement of people, most of them peons from central Chile, provoked long speeches in Congress and several introspective articles by members of the SNA.⁵⁶ Threatened by the prospect of higher wages if workers could not be retained in the agricultural zone, the landowners enlisted the help of the clergy. Directives were sent by the bishops to each parish priest that instructed them to inveigh against the destruction of family life that emigration would cause.⁵⁷

A proposed law to limit the exodus was ignored, however, and the SNA shrank with horror from the suggestion that "coolies" might be brought in to work the fields. There is no evidence that agricultural wages were driven up because of the emigration to Peru. Nor did agriculture suffer; on the contrary, the years between 1868 and 1872 were ones of peak cereal output. Amid the pages of discussion on the *escasez*

⁵³ *BSNA*, 6: 309. A *hacendado* near Peumo, for example, feared that "increased pay would lead the worker to canonize 'San Martes' [St. Tuesday] making it a colleague of 'San Lunes' [St. Monday]." He was referring to the worker's widely deplored habit of taking every Monday off as a kind of informal Saint's Day.

⁵⁴ A group of Chilean students have been at work on a study of the *peón-gañán* (or *afuerino* as the seasonal worker is now known), but the results are so far unpublished. I have had available a working draft of some material by Hugo Zemelman that corroborates these ideas.

⁵⁵ Censuses of 1865 and 1895; Carlos Hurtado, *Concentración de la población y desarrollo económico* (Santiago, 1966), 166–72; 65, quoting Henry Meiggs, *Reseña histórica del ferrocarril entre Santiago y Valparaíso* (Santiago, 1863), 130.

⁵⁶ For a summary of the literature on emigration to Peru, see Watt Stewart, *El trabajador Chileno y los ferrocarriles del Perú* (Santiago, 1939), 5–48; *BSNA*, 2: 286; *Sesiones de la cámara de senadores* (ordinarias), July 31, 1871, pp. 60–67.

⁵⁷ "Circular a los párrocos," *BSNA*, 2: 286–87.

de brazos in the SNA *Boletín*, the always perspicacious Menandier noted that “the peon emigrates for lack of work.”⁵⁸ Indeed others thought that without the safety valve of emigration the rural population would sooner or later “conspire against the established order.”⁵⁹ Emigration from the agricultural zone continued—as may be seen in the nearly constant population of central Chile—while the cities and mining districts grew at a rapid rate. Some Chileans crossed the Andes to acquire small holdings in Mendoza and others worked on the early attempts to dig a Panama canal.⁶⁰

Throughout the years 1865–85 central Chile increased agricultural output and had at its disposal sufficient labor—all without noticeably increasing wages. The explanation for this lies in large part in the changing agrarian structure. Before 1850 central Chile was a land of large estates and few medium and small holdings. A small percentage of the population was settled as service tenants and most of the rest were unattached *peón-gañanes*. Thirty years later many peons had been absorbed into the estates through an extension of *inquilinaje*, but in a situation somewhat less favorable than that of the average *inquilino* before 1850. During these same years (1850–80) many peons began to settle on tiny plots that formed the basis of the large number of *caseríos* and *villorios* (small villages and hamlets) that sprang up in the last third of the century. In addition, a rapid fragmentation of the existing small property occurred.

From these settlements the estates drew labor. An investigation of Caupolicán, for example, reported that the “outside, ambulatory, or loose peons [*peones forasteros, ambulantes, o sueltos*] naturally come from the families of small holders or from the households of *inquilinos*.”⁶¹ For the hacienda it mattered little whether the laborers lived on small plots on the estates or on privately-owned dwarf holdings. In the latter case the estate was spared bothersome administrative problems. In any event, the growth nearby of a large population with too little land to be self-sufficient but content enough for the time being to resist emigration was a welcome development for the landowner.

The changing composition of the population can be seen in the relationship between the more permanently settled groups of *agricultores* (a category that included both proprietors and *inquilinos*) and the more ambulatory *peones* (see table 4). Between 1865 and 1895 the number of *agricultores* increased from 24,000 to 68,000. This reflects, among other things, the extension of *inquilinaje* that took place as grain cultivation spread. The decline in the number of peons from 59,000 to 43,000 was partly a question of censal definition and the result of emigration. The

⁵⁸ BSNA, 13: 164–67. Testimony from a former San Francisco consul was used to point out the “unsuitability” of Asiatic labor. BSNA, 2: 346.

⁵⁹ *Sesiones de la cámara de senadores* (ordinarias), July 31, 1871, p. 63.

⁶⁰ *Primer censo de la República Argentina verificado en los días 15, 16, 17 setiembre, 1869* (Buenos Aires, 1872), 338–50.

⁶¹ BSNA, 2: 387.

TABLE 4. *Agricultores* and *Peones*, Central Chile:
1865, 1895

Year	<i>Agricultores</i>	%	<i>Peones</i>	%
1865	24,000	28%	59,000	72%
1895	68,000	61%	43,000	39%

Source: Population censuses of 1865 and 1895. Data are for the area between the Maipo and Maule Rivers.

changes in both *agricultores* and *peones*, however, point to the changing settlement pattern of rural Chile, a change that was closely related to the rapid fragmentation of small properties.

WITHIN THE COUNTRIES THAT SUPPLIED Europe's food in the nineteenth century, expansion created wholly new agrarian societies in some cases and provoked deep reform in others. In Chile expansion took place within a traditional society and the effect was to strengthen the institutions already present. The century-old system of *inquilinaje* was extended by settling new resident workers while stepping up the labor requirements of older tenants. At the same time part of the floating population was induced to settle near the central valley haciendas. During this reorganization of labor the settlement pattern of rural Chile changed from scattered and even ambulatory homesteads to small hamlets and agrarian villages. From the 1860s on, thousands of new dwarf holdings appeared in central Chile. Further fragmentation of these reduced their proprietors (or their sons) to a choice of part-time work or emigration. Some left for northern mines or railroad construction; others added to the pool of seasonal labor that, in the absence of machinery, was essential to estate agriculture.

As Chile entered the twentieth century Chilean rural society was already cast in the familiar modern pattern of "minifundia-latifundia" symbiosis. On one side were the tiny privately owned plots whose owners, unable to provide subsistence for their families from their own land, found supplementary work on the haciendas. On the other side were the still inefficient large estates encumbered by a more abundant, stable, and desperately poor labor force. This humble mass could neither adequately feed the growing cities nor buy the products of an incipient industrial sector. Rural workers remained isolated from the new currents of Chilean life but they continued to provide the labor and votes that underlay the landowners' dominance of national politics.

Time, Space, and the Geographic Past: A Prospectus for Historical Geography

JOHN A. JAKLE

AS THE GEOGRAPHY of the United States has changed dramatically over the course of the past half century, so too has American academic geography developed anew. Not only has the factual content of the field changed but, more important, the basic viewpoints of geography and the analytical tools of the discipline have evolved. New questions are being asked. How does man perceive his environment and thus define resource alternatives? How do resource perceptions change with time? Given environmental management responses, how do men distribute themselves and their activities in earth space, and how do these distributions change? How does human spatial behavior relate to social interaction? In large measure the principal thrust of academic geography remains the discovery of spatial order as geographers analyze spatial relationships developed between phenomena distributed in earth space; but this modern-day geographical exploration has come to embrace the particular consideration of academic history as well.¹ Geographers are no longer content to study spatial relationship in its static sense but are focusing increasingly on spatial change through time. Accordingly, geographers are expanding their concern with time as well as with space.²

It is my intent in this essay to review briefly the orientation of human

¹ Recent methodological statements concerned with geography's spatial definition include National Academy of Sciences and National Research Council, *The Science of Geography* (Washington, 1965); Peter Haggett, *Locational Analysis in Human Geography* (New York, 1966); and David Harvey, *Explanation in Geography* (London, 1969). The most comprehensive philosophical treatises remain Richard Hartshorne, *The Nature of Geography* (Lancaster, Pa., 1939); and his *Perspective on the Nature of Geography* (Chicago, 1959).

² For discussion of the "time element" in geography and a review of the methodological development of historical geography see Carl O. Sauer, "Foreward to Historical Geography," *Annals of the Association of American Geographers*, 31 (1941): 1-20; A. G. Ogilvie, "The Time-Element in Geography," *Transactions of the Institute of British Geographers*, no. 18 (1952): 1-15; William Kirk, "Historical Geography and the Concept of the Behavioural Environment," *Indian Geographical Journal, Silver Jubilee Edition, 1951* (1952), 152-60; H. C. Darby, "On the Relations of Geography and History," *Transactions of the Institute of British Geographers*, no. 19 (1953): 1-13; Andrew H. Clark, "Historical Geography," in Preston E. James and Clarence F. Jones, eds., *American Geography, Inventory and Prospect* (Syracuse, 1954), 70-105; David Harvey, "Models of the Evolution of Spatial Patterns in Human Geography," in Richard J. Chorley and Peter Haggett, eds., *Models in Geography* (New York, 1967), 549-607.

geography in the United States toward the study of spatial dynamics and to describe the current direction of this effort. Emphasis will be placed on the development of historical geography as the study of man's geographic past. A preliminary word of caution to the reader seems appropriate. This essay should not be construed as a complete review of the literature for it is but a sampling of that literature oriented particularly toward the historical geography of North America, the literature with which I am most familiar. Further, it is not my purpose to re-examine previously debated methodological issues. I would like to sketch briefly the outlines of the growth of historical geography within human geography in order to identify the perspectives of present-day research. The manner in which geographers have sought to relate analytically the dimensions of time and space is the thematic emphasis. Such an approach, it is hoped, may stimulate historians to explore the field of geography more closely than has been done in the past.³ And perhaps this exploration will promote a substantial contribution by historians to historical and human geography as well as stimulate new viewpoints within academic history that are more purely "geographical" in the modern sense.

For historians, in general, the basic concepts of modern geography remain remote; indeed many historians continue to think in purely environmental and even deterministic terms when considering geographic factors in their work. Too many historians in treating geographical considerations narrow their focus to the physical environment and its influence on events, personalities, and the courses of history being narrated. This concentration reflects favorably on the intriguing but now dated arguments of such geographers as Ellen Churchill Semple and Ellsworth Huntington and such historians as Frederick Jackson Turner and Arnold Toynbee, all of whom were influenced to varying degrees by the notions of the German geographer Friedrich Ratzel.⁴ Much has been written criticizing such scholarship for the belief that environment can influence human behavior in a deterministic

³ See, for example, Frederick Jackson Turner, "Report on the Conference on the Relations of Geography and History," *Annual Report, American Historical Association, 1907* (Washington, 1908), 45-48; and Edwin E. Sparks, "Report on the Conference on the Relations of Geography and History," *Annual Report, American Historical Association, 1908* (Washington, 1909), 57-61. For methodological statements written for historians by historical geographers see Jan O. M. Broek, "The Relations between History and Geography," *Pacific Historical Review*, 10 (1941): 321-25; A. H. Clark, "Geographical Change as a Theme for Economic History," *Journal of Economic History*, 20 (1960): 607-17; H. C. Darby, "Historical Geography," in H. P. H. Finberg, ed., *Approaches to History* (Toronto, 1962), 127-56; and Roy H. Merrens, "Historical Geography and Early American History," *William and Mary Quarterly*, 3rd ser., 22 (1965): 529-48.

⁴ Ellen Churchill Semple, *American History and Its Geographic Conditions* (New York, 1903); and her *Influences of the Geographic Environment: On the Basis of Ratzel's System of Anthropogeography* (New York, 1911); see also John K. Wright, "Miss Semple's Influences of Geographic Environment," *Geographical Review*, 52 (1962): 346-61; Ellsworth Huntington, *Mainsprings of Civilization* (New York, 1945); Frederick Jackson Turner, "The Significance of the Frontier in American History," *Annual Report, American Historical Association, 1893* (Washington, 1894), 199-227, reprinted in his *The Frontier in American History* (New York, 1920), 1-38; and his "Geographical Influences in American Political History," *Bulletin of the American Geographical Society*, 46 (1914): 591-95; A. J. Toynbee, *A Study of History* (London, 1947); Friedrich Ratzel,

sense.⁵ It is not the purpose of this discussion to review this debate but only to reiterate the contentions of recent commentators that the physical environment is passive and cannot actively influence human activity.⁶ Careful thought reveals that human behavior is affected by environmental forces only insofar as the environment provides resource alternatives in light of given technologies and perceptual attitudes of the social groups in question. In studying man's relationship to his environment the focus rightfully belongs on man and on his cultural inheritance.

While most historians reject strict deterministic doctrines, many continue to focus exclusively on the physical environment as the sole substance of their geographical consideration.⁷ Although frequently offering elaborate descriptions of the physical environment as the stage upon which historical action develops, more frequently than not these authors fail to treat the human-geographical frameworks within which their historical interpretations are actually set.⁸ Their place-location maps offer little interpretation as to the cultural variations between places, the identifiable regional similarities, or, in a more sophisticated sense, the elements of human spatial interaction or organization. Thus the reader of history is often left without a proper spatial frame of reference in which to place historical explanation.

Fortunately a small number of historical works have been produced that not only provide full geographical reference, but, far more significantly, involve successful attempts at geographical interpretation. Sumner Powell's *Puritan Village* and James Sterling Young's *The Washington Community, 1800-1823* may be cited as examples of a small but viable literature that points the way to meaningful geographical analysis within the context of his-

Anthropogeografie (Stuttgart, 1882-1912); see also H. G. Wanklyn, *Friedrich Ratzel, A Biographical Memoir and Bibliography* (Cambridge, 1961).

⁵ See, for example, James C. Malin, "Space and History: Reflections on the Closed-Space Doctrines of Turner and MacKinder and the Challenge of Those Ideas by the Air Age," *Agricultural History*, 18 (1944): 65-74, 107-26; O. H. K. Spate, "Toynbee and Huntington: A Study in Determinism," *Geographical Journal*, 118 (1952): 406-26; Owen Lattimore, "The Frontier in History," *Relazioni del X Congresso Internazionale di Scienza Storiche*, 1 (1955): 105-38.

⁶ Carl O. Sauer, "The Morphology of Landscape," *California Publications in Geography*, 2 (1925): 19-54, reprinted in John Leighly, ed., *Land and Life: A Selection from the Writing of Carl Ortwin Sauer* (Berkeley, 1963), 315-50; Lucien Febvre, *La terre et l'évolution humaine* (Paris, 1923); George Tatham, "Environmentalism and Possibilism," in Griffith Taylor, ed., *Geography in the Twentieth Century* (Toronto, 1951), 128-62; S. R. Eyre, "Determinism and the Ecological Approach to Geography," *Geography*, 49 (1964): 369-76. See also William O. Koelsch, "The Historical Geography of Harlan H. Barrows," *Annals of the Association of American Geographers*, 59 (1969): 632-51.

⁷ This seems particularly true of undergraduate course and textbook organization at the introductory level. Thus young historians who have come to equate the term "geography" exclusively with notions of physical environment have a bias already developed when they turn to research.

⁸ In the past historical geographers have contributed substantially to the preoccupation of academic history with physical-geographical considerations by focusing on physical geography themselves. See, for example, Semple, *Influences of the Geographic Environment*; Albert Bingham, *Geographic Influences in American History* (Boston, 1903); W. G. East, *The Geography behind History* (New York, 1938); and Derwent Whittlesey, *Environmental Foundations of European History* (New York, 1949).

torical scholarship.⁹ Powell, in treating the diffusion of seventeenth-century social forms from England to colonial Massachusetts, examines the relationships between resource management and social organization. Concerned in part with land tenure practices, he meticulously describes both Old and New World settlement landscapes to base portions of his analysis of social and political conditions on the realities of land division, settlement patterning, and property ownership. Young, in analyzing the early governmental establishment in Washington, D.C., during the Jeffersonian era, pays special attention to the residential distribution of "legislators" and "executives," that is, persons employed in the two respective branches of the federal government. Young writes, "The settlement pattern of a community is, in a sense, the signature that its social organization inscribes upon the landscape, defining the groups of major importance in the life of the community and suggesting the relationships among them."¹⁰

THE LACK OF SPATIAL ORIENTATION displayed in all but a few historical works, although a regrettable development from the geographer's biased point of view, seems a logical result given the pervasive preoccupation of academic history with the time continuum. As geographers deal with earth space, so historians have been traditionally seen as dealing with time as the common denominator of their discipline.¹¹ Although it may be philosophically argued that time and space are integral manifestations (timeless space and spaceless time having been discarded by scientific philosophy), the two separate constructs viewed in a Kantian sense have traditionally been used by geographers, at least, to define geography and history as separate disciplines.¹² It is doubtful, however, that geography can continue its search for spatial understanding by ignoring the integral dictates of time and space as a natural unity; thus have geographers come to focus on the processes of spatial organization through time.

Unlike academic history, geography is considered by its practitioners as essentially a science.¹³ Allied with geology in its formative years in the United

⁹ Sumner Chilton Powell, *Puritan Village* (Wellesley, 1963); James Sterling Young, *The Washington Community, 1800-1823* (New York, 1966).

¹⁰ Young, *Washington Community*, 64.

¹¹ Few books on historical methods discuss the nature of time; perhaps, as Robert Berkhofer points out, "Historians presume a notion of time so fundamental to their studies that they rarely discuss it." Robert F. Berkhofer, Jr., *A Behavioral Approach to Historical Analysis* (New York, 1969), 211. Recent philosophies treating the subject include Nathan Rotenstreich, *Between Past and Present: An Essay on History* (New Haven, 1958); Hans Reichenbach, *The Direction of Time* (Berkeley, 1956); and Adolph Grunbaum, *Philosophical Problems of Space and Time* (New York, 1963).

¹² J. M. Blaut, "Space and Process," *Professional Geographer*, 13 (1961): 1-17.

¹³ Berkhofer discusses academic history as an idiographic discipline from the perspective of the scientific philosophy current in the social sciences. *Behavioral Approach to Historical Analysis*, 243-69. See also Isaiah Berlin, "History and Theory: The Concept of Scientific History," *History and Theory*, 1 (1960): 1-31. For a discussion of geography as a nomothetic discipline see Hartshorne, *Nature of Geography*, 278-305; Fred K. Schaefer, "Exceptionalism in Geography: A

States, geography has continued in its development to be oriented to scientific methodology. As a physical science this orientation has never been seriously in doubt; as a social science, however, the scientific inclinations of human geography have often been obscured by idiographic tendencies. Not until the so-called quantitative revolution of the late 1950s and the more current behavioral revolution has human geography stood firmly in the ranks of the social sciences.¹⁴ Through these two periods of extensive borrowing from mathematics and various fields of the physical, social, and behavioral sciences, human geographers have come increasingly to use mathematical models in research focused on human behavior in its spatial context. In a broader sense these developments have rapidly enlarged the theoretical literature of the discipline and thereby greatly strengthened geography's claim to be a science. Historical geography, whose focus on the geographic past has made it heavily reliant on academic history for stimulus, alone has remained a preserve for the practitioners of humanistic thought and literary persuasion. Not surprisingly it is the social scientists within geography and not the historical geographers who have provided the most recent impetus for reorienting the field toward the study of spatial dynamics. Historical geography has, however, benefited immeasurably from this reorientation, as we shall see below.

The current preoccupation of geography with spatial process is founded in the writings of the early geomorphologists; significant early works include those by William Morris Davis, who developed the concept of "stage" in the cycle of erosion, a development leading to the classification of landforms according to their relative age.¹⁵ Today genetic emphases continue to dominate a physical geography given not only to landform evolution but to the study of ecological succession, soil formation, and climatic dynamics as well.¹⁶ Predicated on this success human geographers developed similar genetic models to describe cultural landscape evolution. Carl Sauer's methodological "Morphology of Landscape" posed a new challenge for geographers by calling for genetic emphasis in the explanation of human land occupancy, as did Derwent Whittlesey's "sequent occupancy" schema, which proposed to emphasize the "stages" of land settling.¹⁷ In academic

Methodological Examination," *Annals of the Association of American Geographers*, 43 (1953): 226-49; and Hartshorne, *Perspective on the Nature of Geography*, 146-72.

¹⁴ See Ian Burton, "The Quantitative Revolution and Theoretical Geography," *Canadian Geographer*, 7 (1963): 151-62; and William Bunge, *Theoretical Geography* (Lund, 1966).

¹⁵ William Morris Davis, "The Geographical Cycle," *Geographical Journal*, 14 (1899): 481-504; and his *Geographical Essays* (Boston, 1909); see also Arthur Strahler, "Dynamic Basis of Geomorphology," *Bulletin of the Geological Society of America*, 63 (1952): 923-38; and S. A. Schumm and R. W. Lichty, "Time, Space and Causality in Geomorphology," *American Journal of Science*, 263 (1965): 110-19.

¹⁶ For general discussions see G. F. Carter and R. L. Pendleton, "The Humid Soil, Process and Time," *Geographical Review*, 46 (1956): 488-507; Arnold Court, "Climatology: Complex, Dynamic, and Synoptic," *Annals of the Association of American Geographers*, 47 (1957): 125-36; and D. R. Stoddart, "Geography and the Ecological Approach: The Ecosystem as a Geographic Principle and Method," *Geography*, 50 (1965): 242-51.

¹⁷ Sauer, "Morphology of Landscape"; Derwent Whittlesey, "Sequent Occupancy," *Annals of the Association of American Geographers*, 19 (1929): 62-65.

history similar developments saw the elaboration of Frederick Jackson Turner's frontier thesis, with its sequences of frontier development, and Walter Prescott Webb's analysis of the institutional phases of resource management on the Great Plains.¹⁸

During the 1930s historical geographers assumed a prime responsibility for the development of methods capable of identifying stages of human occupancy. Historical geographers had originally been content to describe the geography of areas at selected dates in the past. This early approach is exemplified by Ralph Brown's *Mirror for Americans: Likeness of the Eastern Seaboard, 1810*, in which the geography of the United States at that date was reconstructed. Although Brown used as an interesting restriction only those data sources available to scholars in the first decade of the nineteenth century, our attention at present centers on the fact that he ignored analysis of historical change, choosing instead to develop a static view of a past geographical condition.¹⁹ More popular, however, was the development of a diachronic approach whereby geographic change in a selected area was described in historical narrative. Such effort, which continues today, can perhaps be as appropriately termed "geographical history." This latter emphasis may be illustrated by Brown's second opus, the *Historical Geography of the United States*, which is essentially a history of the settlement of land by region.²⁰ Yet effort to identify stages of geographic evolution provided historical geographers with an even greater control over the time dimension for research and expository purposes. Attempts were made to classify or categorize the spatial distributions under study according to selected periodization schemes. Alfred Meyer's study of the Calumet of northwestern Indiana and northeastern Illinois, in which the geography of succeeding periods of land occupancy was described, stands by way of example.²¹

THE MATRIX IN THE DIAGRAM below is introduced to illustrate further what has been said and to lay the basis for much of the subsequent discussion. The matrix attempts to classify various examples of historical and geographical literature selected for discussion in this article according to the manner in which respective authors relate their spatial and time dimensions. The matrix illustrates a basic contention that human explanation operates at various levels of generality along spatial and temporal continuums that are defined by idiographic and nomothetic focuses at the extremes. Thus in reading the diagram one moves along scales of increasing generalization as one proceeds from factual description to classification (that is, periodization or

¹⁸ Turner, "Significance of the Frontier"; Walter Prescott Webb, *The Great Plains, A Study of Institutions and Environment* (New York, 1927).

¹⁹ Ralph H. Brown, *Mirror for Americans: Likeness of the Eastern Seaboard, 1810* (New York, 1948).

²⁰ Ralph H. Brown, *Historical Geography of the United States* (New York, 1948).

²¹ Alfred H. Meyer, "Circulation and Settlement Patterns of the Calumet Region of Northwest Indiana and Northeast Illinois," *Annals of the Association of American Geographers*, 44 (1954): 245-74; 46 (1956): 312-56.

			SPATIAL DIMENSION		
			Idiographic Focus → Nomothetic Focus		
			Areal Description	Regionalization	Spatial Analysis
TEMPORAL	Idiographic Focus ↓	Historical Description	Brown, 1946		
	Nomothetic Focus ↓	Periodization	Turner, 1894 Sauer, 1925 Webb, 1927 Whittlesey, 1929 Meyer, 1954	Clark, 1959 Merrens, 1964 Meinig, 1965 Lemon, 1966	Hagerstrand, 1952
		Historical Change Analysis			

Levels of generalization in the dual treatment of spatial and temporal patterns in historical geography.

regionalization) to the use of broad analytical models. In the diagram Brown's historical geography text is seen to combine what geographers term "areal description" with the description of historical change, while focus on the stages of human occupancy in the works of Turner, Whittlesey, Webb, and Meyer, respectively, combine areal description with more sophisticated approaches to periodization.²²

Beginning in the late 1940s historical geographers took further steps toward the use of general models. First, methods for regionalization were developed. Rather than relying on the overt description of geographical area, these geographers attempted to identify regional patterns inherent in geographical change. For this purpose the "region" was defined as an uninterrupted area displaying some kind of uniformity intensifying toward its core, just as periodization involved "periods conceived as units of historical time possessing a certain focused similarity."²³ Andrew H. Clark's chorological analysis of the early economic development of Prince Edward Island stands as an outstanding example of this integrated temporal and spatial categorization.²⁴ Clark and his students at the University of Wisconsin have been largely responsible for the second development: that of a synchronic

²² Brown, *Historical Geography of the United States*; Turner, "Significance of the Frontier"; Whittlesey, "Sequent Occupance"; Webb, *Great Plains*; Meyer, "Circulation and Settlement Patterns of the Calumet Region."

²³ For a discussion of the regional concept see Derwent Whittlesey, "The Regional Concept and the Regional Method," in James and Jones, *American Geography*, 19-69.

²⁴ Andrew H. Clark, *Three Centuries and the Island: A Historical Geography of the Settlement and Agriculture in Prince Edward Island* (Toronto, 1959).

approach involving the use of geographical inventories for selected cross sections in time in order to identify and explain geographical change through time. The selection of dates for comparative purposes in such work, however, has not always incorporated the rationales for periodization but has tended to reflect the availability of data at otherwise unrelated dates. James Lemon's work on the rural geography of southeastern Pennsylvania during the eighteenth century stands as an excellent example of this effort whereby the static geography at selected points in time is compared across time and the observed changes analyzed.²⁵ Until the recent willingness of historical geographers to adopt descriptive and inferential statistics in their work such analyses were most often accomplished by the use of cartographic techniques whereby spatial distributions were mapped and compared visually in order to discover regional patterns of spatial covariation or regional patterns of changing spatial covariation.

The period of the 1940s and 1950s was also characterized by a growing concern among historical geographers to adopt and perfect the techniques of modern historiography that were applicable to their work. A particular emphasis focused on the validation of historical fact as historical geographers extended their interest to a wide range of topical problems. In retrospect it would seem that efforts to borrow from academic history may have had a separating influence, isolating many historical geographers from changes sweeping the remainder of human geography. This separating influence has been particularly apparent in the reluctance of many historical geographers to consider quantitative techniques of spatial analysis and theory development in their work.

Through the 1940s and 1950s concern in human geography with geographical change through time was subordinated to the study of cultural and economic patterns in space. Cultural geography and economic geography emerged as the major subdivisions within human geography, and historical geographers tended to orient themselves toward one or the other of these emphases.²⁶ The lesser drive, that of cultural geography, involved the application of the idea, or concept, of "culture" to geographic problems.²⁷ Charles F. Gritzner explains that

Cultural geography begins with the anthropological concept of culture, considers culture traits and cultural groups in terms of their historical development, weighs

²⁵ James T. Lemon, "A Rural Geography of Southeastern Pennsylvania during the Eighteenth Century: The Contributions of Cultural Inheritance, Social Structure, Economic Conditions, and Physical Resources" (Ph.D. dissertation, University of Wisconsin, 1964); and his "The Agricultural Practices of National Groups in Eighteenth-Century Southeastern Pennsylvania," *Geographical Review*, 56 (1966): 467-96.

²⁶ For a highly readable review of the two schools of human geography in the 1940s and 1950s see James D. Clarkson, "Ecology and Spatial Analysis," *Annals of the Association of American Geographers*, 60 (1970): 700-16.

²⁷ Some of the more recent methodological statements concerning cultural geography include William L. Thomas, Jr., "Studies in Cultural Geography," in *The Science of Geography, Report of the Ad Hoc Committee On Geography, Earth Science Division, National Academy of Sciences, National Research Council* (Washington, 1965), 23-31; and Charles F. Gritzner, Jr., "The Scope of Cultural Geography," *Journal of Geography*, 65 (1966): 4-11.

the interactions between culture and the physical landscape, interprets cultural landscapes, and, in the final analysis, through the mapping of traits and trait complexes on the earth, provides a sound basis for the division of earth space into culture regions and subregions.²⁸

Born in anthropology, the culture concept won widespread endorsement among geographers who were interested in primitive societies and in peoples of the historic and prehistoric past. Perhaps the geographers' best effort in a historical vein has brought a modified ecological approach to the study of man in his past adjustment to environment. The work on plant and animal domestication is particularly recommended for review.²⁹

The largest literature built around the culture concept focuses on geographical patterns of human settlement resulting from man's utilization of the earth's surface. This literature covers a wide variety of topics, such as the regional distribution of folk and vernacular architecture, the diffusion of settlement forms, and the evolution of urban morphological patterns.³⁰ Recent technological development, especially in the fields of remote sensing and computer mapping, promise even greater emphasis on settlement research.³¹ Remote sensing techniques should continue to be important to historical geographers for the discovery and mapping of relict settlement features that are detectable only with airborne instrumentation.³² Such innovation should also benefit historians who are similarly concerned with the interpretation of landscape change, or what is often called "topographic history."³³

²⁸ Gritzner, "Scope of Cultural Geography," 10.

²⁹ For a general introduction to cultural geography see Philip Wagner, *The Human Use of the Earth: An Examination of the Interaction between Man and His Physical Environment* (New York, 1960). The literature on early man and plant and animal domestication is best exemplified by George F. Carter, *Plant Geography and Cultural History in the American Southwest* (New York, 1945); Carl O. Sauer, *Agricultural Origins and Dispersals: The Domestication of Animals and Foodstuffs* (New York, 1952); and Karl W. Butzer, *Environment and Archeology: An Introduction to Pleistocene Geography* (Chicago, 1964).

³⁰ For an overview of the literature of settlement geography see William L. Thomas, Jr., ed., *Man's Role in Changing the Face of the Earth* (Chicago, 1956). Outstanding examples in the areas of folk architecture and urban morphology given to diffusion problems are, respectively, Fred Kniffen, "Folk Housing: Key to Diffusion," *Annals of the Association of American Geographers*, 55 (1965): 549-77; and Dan Stanislawski, "The Origin and Spread of the Grid-Pattern Town," *Geographical Review*, 36 (1946): 105-20.

³¹ For an introduction to remote sensing see J. C. Sherman, "Accumulation of Geographic Data through Remote Sensing," *Proceedings of the Second Symposium on Remote Sensing of the Environment* (Ann Arbor, 1963), 427-29; Robert B. Simpson, "Radar, Geographic Tool," *Annals of the Association of American Geographers*, 56 (1966): 80-96; and John E. Estes, "Some Application of Aerial Infrared Imagery," in *ibid.*, 673-83. For an introduction to computer mapping see Duane F. Marble, *Some Computer Programs for Geographic Research* (Evanston, 1967); and Laboratory for Computer Graphics and Spatial Analysis, *The Synagraphic Computer Mapping Program: SYMAP (Version V)* (Cambridge, Mass., 1968).

³² H. L. Cameron, "History from the Air," *Photogrammetric Engineering*, 24 (1958): 366-75; John K. S. St. Joseph, ed., *The Uses of Air Photography: Nature and Man in a New Perspective* (New York, 1966).

³³ Excellent examples of topographical description include John Atlee Kouwenhoven, *The Columbia Historical Portrait of New York: An Essay in Graphic History in Honor of the Tri-centennial of New York City and the Bicentennial of Columbia University* (Garden City, 1953); Sam B. Warner, Jr., *Streetcar Suburbs: The Process of Growth in Boston, 1870-1900* (Cambridge,

Although cultural geography has made a considerable contribution, orientation toward economic explanation has proven to be the strongest force within human geography and thus within historical geography. After World War I economic geography had evolved as both an alternative to purely physical-geographical interpretations and as a reaction against the tenets of environmental determinism. Bolstered by a wealth of production data obtained from international, national, and corporate data sources, the field had by World War II come to focus on the locational rationales that underlay the geographical distribution of economic production.³⁴ With a substantial contribution from the field of economics geographers by the mid-1950s were utilizing theoretical models offering explanations for a variety of locational problems.³⁵ Foremost were "central place," industrial location, and land rent theories, all given to explaining the spatial distribution of economic activities: the arrangement of market centers, the location of industrial facilities, and the distribution of land uses in both rural and urban contexts respectively.³⁶ Although this work was narrowly conceived within the bounds of purely cost-oriented or economic terms and did not often admit the significance of politically and socially motivated behavior, it served the purpose of developing for human geography its first integrated literature closely interwoven by the threads of theoretical pursuit.

Emphasis on model building and the growth of theory in economic geography required more precise means of measuring, summarizing, and relating observations that only the coding and manipulating of information

Mass., 1962); and Walter M. Whitehill, *Boston: A Topographical History* (Cambridge, Mass., 1968). The recent collaborative work by a geographer and a historian deserves special mention: Harold M. Mayer and Richard C. Wade, *Chicago: Growth of a Metropolis* (Chicago, 1969).

³⁴ For a review of the early development of economic geography see Raymond E. Murphy, "The Fields of Economic Geography," in James and Jones, *American Geography*, 240-45; Harold H. McCarty, "Agricultural Geography," in *ibid.*, 258-77; Raymond E. Murphy, "The Geography of Mineral Production," in *ibid.*, 278-91; Chauncy D. Harris, "The Geography of Manufacturing," in *ibid.*, 292-309; and Edward L. Ullman, "Transportation Geography," in *ibid.*, 310-33.

³⁵ The trend toward the use of models in geography is well documented in Chorley and Haggett, *Models in Geography*; see especially Peter Haggett and Richard J. Chorley, "Models, Paradigms, and the New Geography," in *ibid.*, 19-41.

³⁶ Central place theory deals with the distribution of service and trade activities in a spatially defined system of trade centers, called "central places." It grew primarily from the work of Walter Christaller; see Walter Christaller, *Die Zentralen Orte in Suddeutschland* (Jena, 1933), tr. and ed. by Charlisle W. Baskin as *Central Places in Southern Germany* (Englewood Cliffs, 1966). For a recent review of current research see Brian J. L. Berry, *Geography of Market Centers and Retail Distribution* (Englewood Cliffs, 1967). Industrial location theory focuses primarily on the economic costs of locating industrial activity within a spatially defined system of productive resources and markets. Classics in the literature include August Losch, *Die Raunliche Ordnung der Wirtschaft* (Jena, 1941), tr. and ed. by William H. Woglom and Wolfgang F. Stolper as *The Economics of Location* (New Haven, 1952); Edgar M. Hoover, *The Location of Economic Activity* (New York, 1948); and Walter Isard, *Location and Space-Economy* (Cambridge, Mass., 1956). Land rent theory offers explanation for land use in both urban and rural settings primarily in economic-cost terms. See Michael Chisholm, *Rural Settlement and Land Use* (London, 1962); and William Alonso, *Location and Land Use* (Cambridge, Mass., 1964). The full spectrum of economic spatial theory is concisely reviewed in two recent works: Martin Bechmann, *Location Theory* (New York, 1968); and Hugh O. Nourse, *Regional Economics, A Study in the Economic Structure, Stability, and Growth of Regions* (New York, 1968).

through quantification could provide. Initially quantification was largely restricted to the use of "correlative" techniques whereby the spatial covariation of distributions could be compared and analyzed without recourse to the more subjective cartographic comparisons.³⁷ Much of this work suffered, however, from its structural or distributional orientation, as emphasis was usually restricted to the description and comparison of static geographical distributions representing the results of human behavior rather than being focused on the actual spatial behaviors of the human actors involved. Nonetheless this effort did enable human geographers tentatively to identify significant distributional regularities, which have suggested, in turn, behavioral patternings calling for more direct investigation.³⁸

The few historical geographers to engage in model building tended not to focus at the operational level of hypothesis substantiation, as most economic geographers had done; rather they limited their efforts to more speculative conceptual levels. Although their models attempted to identify processes of spatial organization by focusing on geographical distributions as they were seen to be dynamic and to change through time, few models were even remotely capable of substantiation in an operational context of rigorous measurement and testing. In addition most of these efforts suffered from a continued adherence to preconceived notions of "stage" as a means of handling the time dimension, for, in most instances, spatial change continued to be conceptualized either in terms of periods or across arbitrarily selected time intervals. It should be realized, of course, that the best of these theoretical contributions, such as Donald Meinig's analysis of the evolving Mormon culture region of the western United States and Allen Pred's study of the spatial dynamics of nineteenth-century American industrial growth, were but exploratory efforts that offered direction rather than definitive elaboration.³⁹ Meinig identified stages by which cultural traits

³⁷ For an excellent discussion of the relationship between cartographic and descriptive statistical techniques placed in the context of economic and human geography see Harold H. McCarty and James B. Lindberg, *A Preface to Economic Geography* (Englewood Cliffs, 1966). For texts that will introduce the reader to techniques of quantitative spatial analysis see, in ascending order of sophistication, John P. Cole and Cuchlaine A. M. King, *Quantitative Geography: Techniques and Theories in Geography* (New York, 1968); Brian J. L. Berry and Duane F. Marble, eds., *Spatial Analysis: A Reader in Statistical Geography* (Englewood Cliffs, 1968); and Leslie J. King, *Statistical Analysis in Geography* (Englewood Cliffs, 1969).

³⁸ An important regularity investigated in human geography has been that of "distance decay," which states that the potential for human interaction across space declines as distance between interactors increases. Much of central place, industrial location, and land rent theory incorporates this notion as a basic premise, particularly in the prediction of potential interaction between places using variants of the simplistic equation $I_{ij} = [f(P_i P_j)/f(D_{ij})]$, where I_{ij} = interaction between centers i and j , P_i and P_j = the size of the two respective interacting populations, and D_{ij} = separating distance. Borrowed from Newton's law of gravitation and sometimes called a "gravity" or "potential" model, the interaction concept is often used to establish a normative or expected situation that is then contrasted with empirically derived patterns of spatial interaction in a search for patterns of regular deviation. For short reviews of the procedure see Gerald A. P. Carrothers, "An Historical Review of the Gravity and Potential Concepts of Human Interaction," *Journal of the American Institute of Planners*, 22 (1956): 94-102; and Gunnar Olsson, *Distance and Human Interaction: A Review and Bibliography* (Philadelphia, 1965).

³⁹ Donald W. Meinig, "The Mormon Culture Region: Strategies and Patterns in the Geography

diffused in earth space to produce spatial variations in the human occupancy of land, while Pred, following W. W. Rostow and particularly Gunnar Myrdal, developed a model for viewing economic development in a spatial framework.⁴⁰

MUCH OF THE EARLY use of theory in human geography depended upon models that can best be described as deterministic in nature. Deterministic modeling postulated direct cause-and-effect relationships between variables, eliminating from consideration human behavior due to chance or to irrationality. Thus most economic-geographic models were built around such assumptions as that of least-cost behavior, where, for example, industrial location decisions hinged on completely rational efforts to minimize the costs of production and distribution, or, in other words, to select locations that enabled maximization of profits. In this regard many human geographers placed themselves in a framework of economic determinism quite as restrictive as the environmental determinism of a previous era. Working independently, however, was the Swedish geographer Torsten Hagerstrand, who turned to the use of stochastic devices whereby the spatial-distributional results of human behavior, both economically rational and otherwise, could be simulated in time and space by using mathematical laws of probability.⁴¹ Hagerstrand's "Monte Carlo" techniques were applied first to the study of human migrations in earth space and then to the study of innovation diffusion in order to provide a rudimentary research construct intended to describe spatial-distributional change in both its spatial and time contexts.⁴²

In brief, simulation approaches permit a step-by-step generation of spa-

of the American West, 1847-1964," *Annals of the Association of American Geographers*, 55 (1965): 191-220; and his "Perspective on the American West: An Analytical Framework for Historical Geography," paper read at the annual meeting of the Association of American Geographers, Aug. 31, 1966, in Toronto; Allen R. Pred, *The Spatial Dynamics of United States Urban-Industrial Growth, 1800-1914* (Cambridge, Mass., 1966).

⁴⁰ Walter W. Rostow, *The Stages of Economic Growth: A Non-Communist Manifesto* (Cambridge, Mass., 1960); Gunnar M. Myrdal, *Rich Lands and Poor: The Road to World Prosperity* (New York, 1957). An excellent review of the spatial literature of economic development appears in D. E. Keeble, "Models of Economic Development," in Chorley and Haggett, *Models in Geography*, 243-302.

⁴¹ Torsten Hagerstrand, "Migration and the Growth of Culture Regions," in his *Studies in Rural-Urban Interaction* (Lund, 1951); his *The Propagation of Innovation Waves* (Lund, 1952); his *Innovationsforsloppet ur Korologisk Synpunkt* (Lund, 1953), tr. by Allen Pred as *Innovation Diffusion as a Spatial Process* (Chicago, 1968); and Hagerstrand, "Migration and Area," in *Migration in Sweden, A Symposium* (Lund, 1957). For a concise introduction to Hagerstrand's ideas, see his "A Monte Carlo Approach to Diffusion," *Archives Europeenes de Sociologie*, 6 (1965): 43-67. The basic characteristic of deterministic models is that the development of some system in time and space can be completely predicted provided a set of initial conditions and relationships are known. Probabilistic or stochastic models, on the other hand, build random variables into their structure. Harvey provides an excellent summary of the wide spectrum of models, both deterministic and stochastic, developed for historical-geographical research. "Models of the Evolution of Spatial Patterns in Human Geography," 561-62.

⁴² Hagerstrand, "Migration and the Growth of Culture Regions" and *Propagation of Innovation Waves*. For excellent reviews of the literature of spatial diffusion see Lawrence A. Brown, *Diffusion Dynamics: A Review and Revision of the Spatial Diffusion of Innovation* (Philadelphia, 1968); and Peter R. Gould, *Spatial Diffusion* (Washington, 1969).

tial distributional patterns within the context of a game situation. Here a contrived set of rules based on empirical observation outlines both a probabilistic sequence for action and a spatial action field for simulating events in time and space. The purpose is to produce mapped distributions, which are then compared with empirically derived patterns not to determine the degree of conformity between patternings but to establish degrees of mutual similarity. If the generated distributions approach reality in such spatial terms as extent, dispersion, density, and directional bias, then the model builder is assured that the rules of his game reasonably approximate the decision-making contexts of society in which the real human behavior was produced. It should be emphasized that the search for periods or stages of historical action has continued in research on spatial diffusion. Here researchers, in analyzing the spatiotemporal distributional characteristics of a population's decision to adopt an innovation, most often resort to logistic curves along which periods of early innovation, early majority adoption, late majority adoption, and laggard acceptance of a newly introduced idea are identified. These observations are then reintroduced into models determining the rules for the sequencing of gaming action.

Simulation modeling has been considered by many of its practitioners as presenting a process orientation as opposed to the static distributional or structural emphasis traditional in geography.⁴³ It is true that simulation work has increased the capabilities of geography for describing dynamic spatial distributions, but this has not, for the effort, brought the discipline any closer to understanding the real rationales underlying human spatial behavior except insofar as behavioral contexts and probabilities of action can be reasonably validated after their fact of discovery. Such effort cannot be considered as attacking directly the processes of spatial behavior, for the means for directly understanding man's perception and/or cognition of his alternatives of spatial action within his environment are lacking.

Although simulation techniques used in innovation-diffusion and migration work in geography suffer from a lack of direct behavioral approach, they suggest a means for placing the analysis of spatial distribution in a truly dynamic framework. Of course this capability can only be measured in relation to efforts of the past. A reference to Hagerstrand's simulation research has, therefore, been placed in the descriptive matrix of the diagram. Unfortunately the simulation work done by geographers, although embracing the temporal dimension, has not given an equal consideration to it. If gaming techniques have sought to simulate occurrences in space, they have not been as deliberately concerned to simulate occurrences in time. As previously suggested, assumptions of stage or period are relied

⁴³ Gould, *Spatial Diffusion*, 1; Richard L. Morrill, "The Shape of Diffusion in Space and Time," *Economic Geography*, 46 (1970): 259-68; and his "The Development of Spatial Distributions of Towns in Sweden: An Historical-Predictive Approach," *Annals of the Association of American Geographers*, 53 (1963): 1-14.

upon in the creation of game rules by which activities or phenomena are assigned simulated spatial positions. Here is where historians might contribute through the analysis of human timing; research given to explaining man's propensity to space activities or events in the temporal dimension.⁴⁴ Such historical analysis could provide builders of spatial models with more valid probabilities of historical action. As the diagram implies, such analysis in geography and history has yet to begin.

Despite the sophistication of spatial diffusion techniques as manipulative devices, these techniques simulate spatiotemporal patterns of human behavior only insofar as model builders are able to assign valid probabilities to choices within validly conceived gaming contexts. This, in turn, necessitates an understanding of the basic social and psychological structures of human communication. Therefore, how do human beings obtain, possess, and transmit an awareness of human behavioral alternatives? How are these options valued; how do actions based on these evaluations derive? For the geographer a greater understanding of man's cognition of spatial alternatives within his environment seems imperative; for the historical geographer focus on the human cognition of spatial options in the historical past seems necessary. It is toward this behavioral emphasis of both human and historical geography that present research tends.

HISTORICAL GEOGRAPHERS HAVE, indeed, traditionally been concerned with environmental perception, although their efforts, as in the whole of human geography, have focused more on the results of resource management than on the requisite perceptual and decision-making processes. Relatively few studies have been undertaken with the specific intention of exploring what the English geographer William Kirk has called the "behavioural environment." Kirk wrote early in the 1950s:

In as much as in historical geography we are concerned with the behaviour of human groups in relation to environment it behooves us to reconstruct the environment not only as it was at various dates but as it was observed and thought to be, for it is in this behavioural environment that physical features acquire values and potentialities which attract or repel human action.⁴⁵

Among the studies that embrace Kirk's proposition is Brown's geographic reconstruction of the American Eastern seaboard of 1810. Brown assumed the intellectual stance of a contrived but not unlikely Philadelphia mer-

⁴⁴ Through their work on business cycles economists have already made substantial contributions toward a general theory of "human timing." For examples of analyses of time series see Clive W. J. Granger and Michio Hatanaka, *Spectral Analysis of Economic Time Series* (Princeton, 1964).

⁴⁵ William Kirk, "Historical Geography and the Concept of the Behavioural Environment," 159. Similar sentiments were being expressed in the United States; see John K. Wright, "Terrae Incognitae: The Place of Imagination in Geography," *Annals of the Association of American Geographers*, 37 (1947): 1-15.

chant of the period in order to view the geography of the United States through the eyes of a hypothesized historical actor. Perhaps less subjective in its approach is the excellent description and analysis of the Indian boundary line in the Southern colonies from 1763 to 1775 by Louis DeVorsey.⁴⁶ Here the author treats the boundary line as a geopolitical device capable of balancing the often conflicting interests of the American Indian, the frontier settler, and the British Board of Trade. Of particular value is DeVorsey's attempt to describe the decision-making processes that underlay the creation of the ambitious boundary project. Translating a wide range of economic and social motivation into varying courses of political expediency, the author brings a behavioral interpretation to his treatment of a historical-geographical phenomena. Other outstanding works in which authors attempt to interpret decision making in the contexts of past behavioral environments include Sauer's evaluation of the Spanish system of resource exploitation in sixteenth-century Caribbean America, Clark's interpretation of spatial organizational schemes in seventeenth-century Acadia, and Meinig's exploration of European and American resource perceptions on the Great Columbia Plain.⁴⁷

Although consideration of the behavioral environment has been implicit in much of the recent work in historical geography, relatively few studies have been focused directly on the perception or cognition theme. Important exceptions include the analyses by G. Malcomb Lewis and Martyn Bowden on the changing perception of the American Great Plains and Merrens' evaluation of sixteenth- and seventeenth-century published accounts of colonial South Carolina.⁴⁸ Significant also is the work by Yi-Fu Tuan that treats various culture groups and the growth of their prevailing attitudes toward nature. His recent epistemological treatment of man and his perception of physical landscape evolution, particularly the process of geomorphology, is especially interesting.⁴⁹

It should be noted in passing that humanists in other disciplines have also begun the analytical pursuit of man's perception of past behavioral environments. This effort has been undertaken largely outside the postu-

⁴⁶ Louis DeVorsey, Jr., *The Indian Boundary in the Southern Colonies, 1763-1775* (Chapel Hill, 1966).

⁴⁷ Carl O. Sauer, *The Early Spanish Main* (Berkeley, 1966); Andrew H. Clark, *Acadia: The Geography of Early Nova Scotia to 1760* (Madison, 1968); Donald W. Meinig, *The Great Columbia Plain, 1805-1910* (Seattle, 1968).

⁴⁸ G. Malcomb Lewis, "Changing Emphases in the Description of the Natural Environment of the American Great Plains Area," *Transactions of the Institute of British Geographers*, 30 (1962): 75-90; his "Regional Ideas and Reality in the Cis-Rocky Mountain West," in *ibid.*, 38 (1966): 135-50; and his "William Gilpin and the Concept of the Great Plains Region," *Annals of the Association of American Geographers*, 56 (1966): 33-51; Martyn J. Bowden, "The Perception of the Western Interior of the United States, 1800-1870: A Problem in Historical Geography," *Proceedings of the Association of American Geographers*, 1 (1969): 16-21; H. Roy Merrens, "The Physical Environment of Early America: Images and Image Makers in Colonial South Carolina," *Geographical Review*, 59 (1969): 530-56.

⁴⁹ Yi-Fu Tuan, "Man and Nature," *Landscape*, 15 (1966): 30-36; and his *The Hydrological Cycle and the Wisdom of God: A Theme In Geoteleology* (Toronto, 1968).

lates of geographic theory for spatial preoccupations have been largely absent; nonetheless exciting perspectives potentially applicable to spatial research have accrued. Particularly significant are the analyses of American literature such as Henry Nash Smith's *Virgin Land: The American West as a Symbol and Myth* and Leo Marx's *The Machine in the Garden*, both pointed toward an understanding of past environmental evaluations that have been important in the course of the nation's cultural evolution. Suggestive, perhaps, of future work to come in academic history are Roderick Nash's *Wilderness and the American Mind* and Peter Schmitt's *Back to Nature: The Arcadian Myth in Urban America*.⁵⁰

Within the broad context of human geography the most vigorous theoretical research on environmental perception has focused on man's awareness of environmental hazards.⁵¹ Beginning in the 1950s geographers looked first at the human occupancy of floodplain areas subject to periodical flooding.⁵² Awareness, or lack of awareness, of potential flooding and the willingness to accept varying degrees of property risk were seen as the major influence on the practice of floodplain management. Also significant were analyses of agriculture on the Great Plains as viewed through the farmer's decision to irrigate—given his perception of drought potential—the public official's perception of snow hazard as it underlay capital expenditures for snow removal, and the use of game theory to analyze land use and cropping patterns with regard to climatic awareness.⁵³ Of particular interest to the historical geographer and to the historian was the realization that both timing and areal spacing of environmental events and human activities greatly influence human awareness of environmental phenomena in problem-solving contexts.⁵⁴

Much of the research on environmental perception, although treating environment in a behavioral framework, has only indirectly posed questions of purely spatial implication. It might be postulated that this failing

⁵⁰ Henry Nash Smith, *Virgin Land: The American West as a Symbol and Myth* (New York, 1957); Leo Marx, *The Machine in the Garden: Technology and the Pastoral Idea in America* (New York, 1964); Roderick Nash, *Wilderness and the American Mind* (New Haven, 1967); Peter J. Schmitt, *Back to Nature, The Arcadian Myth in Urban American* (New York, 1969).

⁵¹ See Ian Burton and Robert Kates, "The Perception of Natural Hazards in Resource Management," *Natural Resources Journal*, 3 (1964): 412-41.

⁵² Gilbert F. White *et al.*, *Papers on Flood Problems* (Chicago, 1961); Robert W. Kates, *Hazard Perception in Flood Plain Management* (Chicago, 1962); Ian Burton *et al.*, *The Shores of Megalopolis: Coastal Occupance and Human Adjustment to Flood Hazard* (Elmer, N.J., 1965).

⁵³ Thomas Saarinen, *Drought Perception in the Great Plains* (Chicago, 1966); John F. Rooney, Jr., "The Urban Snow Hazard in the United States: An Appraisal of Disruption," *Geographical Review*, 57 (1967): 538-59; Peter Gould, "Man against His Environment: A Game Theoretic Framework," *Annals of the Association of American Geographers*, 53 (1963): 290-97. An example of recent efforts to utilize techniques of attitude scaling developed in psychology may be found in Stephen Goland and Ian Burton, "A Semantic Differential Experiment in the Interpretation and Grouping of Environmental Hazards," *Geographical Analysis*, 2 (1970): 120-34.

⁵⁴ For an introduction to research in environmental perception see David Lowenthal, ed., *Environmental Perception and Behavior* (Chicago, 1967). For a concise review of the literature of perception in geography see L. J. Wood, "Perception Studies in Geography," *Transactions of the Institute of British Geographers*, 50 (1970): 129-42.

stems from a pervasive reluctance by human geographers to abandon their discipline's spatial-distributional or structural emphases. Thus human geographers have been more concerned to interpret spatial patterns resulting from human behavior by applying economic, cultural, and, more recently, political and social behavioral rationales than they have been to analyze spatial behavioral processes in order to interpret patterns observed by economists, anthropologists, political scientists, sociologists, and colleagues in other disciplines. Emerging, however, is a new research alternative that is given more specifically to man's perception of place and distance and more generally to man's capabilities for space searching. These concerns more directly assault man's cognitive representation of, his reactions to, and his actions within his spatial environment. These new emphases should have a considerable impact on the future evolution of human and historical geography.

The concepts of territoriality, proxemic behavior, mental mapping, and symbolic meaning in landscape, which have been developed in ethology, psychology, anthropology, and urban planning as well as in geography, narrow the focus of environmental perception to more purely spatial considerations in the search for a general theory of spatial behavior. Territoriality—the definition and defense of surrounding spaces by animals—is seen to promote such varied human spacing mechanisms as the ghettoization of minority groups in urban space and the establishment of sovereignty by nation states over national territories.⁵⁵ Closely related are notions of proxemic behavior whereby individuals define and manipulate spaces for communicative purposes.⁵⁶ Initially formulated within geography by the French social geographer Maximilien Sorré, notions of proxemic behavior have been applied to problems of environmental design particularly in the work of the urban planner Paul-Henry Chombart de Lauwe.⁵⁷ Analysis of the usage of personal space within the micro-scale contexts of hospital wards, classrooms, and libraries has suggested studies of group geography or room ecology that imply a negation of the traditionally exclusive adherence to geographical analysis at world, national, and/or regional scales.⁵⁸ The study of meaning in landscape, illustrated by the

⁵⁵ See C. R. Carpenter, "Territoriality: A Review of Concepts and Problems," in Anne Roe and George C. Simpson, eds., *Behavior and Evolution* (New Haven, 1958), 224–50; and Stanford M. Lyman and Marvin B. Scott, "Territoriality: A Neglected Sociological Dimension," *Social Problems*, 44 (1967): 236–49. For an attempt to apply the concepts of social territory to human sociological problems see Gerald D. Suttles, *The Social Order of the Slum* (Chicago, 1968); and Sanford Labovitz, "Territorial Differentiation and Societal Change," *Pacific Sociological Review*, 8 (1969): 70–75.

⁵⁶ See Edward T. Hall, "Proxemics: The Study of Man's Spatial Relations," in Iago Gladston, ed., *Man's Image in Medicine and Anthropology* (New York, 1963), 422–45; and Hall, *The Hidden Dimension* (New York, 1966).

⁵⁷ Maximilien Sorré, *Rencontres de la Géographie et de la Sociologie* (Paris, 1957); Paul-Henry Chombart de Lauwe et al., *The Sociology of Housing: Research Methods and Future Perspectives* (Rotterdam, 1959). An excellent review of the conceptualization of social space by Sorré and Chombart de Lauwe is to be found in Anne Buttner, "Social Space in Interdisciplinary Perspective," *Geographical Review*, 59 (1969): 417–26.

⁵⁸ Robert Sommer, *Personal Space: The Behavioral Basis of Design* (Englewood Cliffs, 1969).

planner's concern with the human perception of urban forms and the geographer's concern with the symbolic meaning of places (undertaken in the study of human migration, reciprocal travel in activity spaces, and a host of traditional locational problems) has added further stimulus.⁵⁹ From this research it is hoped that general theories of spatial behavior will emerge to provide the social and behavioral sciences with a wholly new approach to the understanding of human life, an approach in which the study of human spacing and spatial movement assumes real predictive powers. It should be admitted, however, that such universal generalization lies in the future.

If human geography reorients itself to the study of spatial behavior wholly defined in its own process terms, spatial analysis will be liberated from a conceptual reliance on the study of nonspatial process systems. Yet spatial analysis cannot and will not function exclusively unto itself, for new conceptual links to other behavioral systems are necessary. Primary among these integrating concepts is the hypothesized dualism of spatial and social order through separation. From his study of the social behavior of animals John B. Calhoun has suggested that spatial segregation and social stratification may operate as an integral for satisfying the human need for separation. Thus it has been observed that when society is formally structured along class lines, the need for group spatial separation declines; conversely, when social stratification relaxes, persons of like class seek mutual reinforcement through the safety of social-territorial tendencies.⁶⁰ Although unsupported by empirical research, this hypothesis nonetheless provides a substantial research challenge with far-reaching implications for environmental design.

IT IS NOT YET CLEAR whether human geography will evolve as a science of spatial behavior restricted to purely spatial concerns or whether the broader considerations of environmental perception and resource management will prevail. The most recent theoretical inputs, as discussed above, would seem to indicate that the discipline is presently inclined toward the former, although present emphases may be but another passing phase as human geographers grapple to identify the core of their discipline. Critics of geography have quipped in the past that "geography is what geographers do." To this cliché might be added the observation that geog-

⁵⁹ For an introduction to symbolic meaning in landscape see Derk DeJonge, "Images of Urban Areas: Their Structure of Psychological Foundations," *Journal of the American Institute of Planners*, 53 (1962): 266-76; and Donald Appleyard, Kevin Lynch, and John R. Meyer, *The View from the Road* (Cambridge, Mass., 1960). For analysis of place symbols in the context of migration and reciprocal travel behavior see Peter R. Gould, "On Mental Maps," *Michigan Inter-University Community of Mathematical Geographers*, discussion paper no. 9 (Ann Arbor, 1966); and his "Structuring Information On Spatio-Temporal Preferences," *Journal of Regional Science*, 9 (1967): 260-74; and David L. Huff, "A Topographical Model of Consumer Space Preferences," *Papers and Proceedings of the Regional Science Association*, 6 (1960): 159-73.

⁶⁰ John B. Calhoun, "The Role of Space in Animal Sociology," *Journal of Social Issues*, 22 (1966): 46-58.

raphers do what academia and society encourage. If geographers can obtain from pure spatiobehavioral emphases satisfaction in the academic community and practical input to problem solving in society at large, then research so oriented will likely thrive. Society in the United States, endowed with an extensive land resource, has been slow to exercise community control of earth-space utilization. As Americans continue to concentrate in urban places, the realization that earth space must be carefully managed as a limited resource may grow. If so, geographical expertise, focused on the mechanisms of human spacing, may prove to be valued most highly.

Yet today two major approaches can be clearly identified in human geography: the study of human spatial behavior with implications for human environmental organization, and the study of human environmental management reduced to its spatial dimensions. The first emphasis derives from a recent realization, both within the field of geography and outside it, that human awareness of spatial alternatives of action is important to society's conduct of everyday life. Research continues in the belief that the structuring of the human environment must reflect this basic fact. The second emphasis stems from the traditional charge of geography to relate man and his environment systematically. Spatial variation in the human management of environment provides a focus for research commensurate with the earlier concern of geography with place description. Perhaps the future will bring a synthesis of these two approaches and produce a science of man's "spatial environment."

It is not yet clear what role historical geographers will play in the future. Certainly they will be inclined toward the application of spatial-environmental theory to the interpretation of past geographical realities. It is hoped that they will participate directly in the development of dynamic models to explain human environmental awareness and derived resource management and spatial behaviors. At present historical geography has not assumed a definitive stance, for this field, also reflecting dynamic trends within and without, is still very much in the making.

In the future historians and geographers will share vital common ground. Perhaps this common territory will be the historical geography of the future. If geographers are to refine their models of past spatial change, historical contexts, especially that of the behavioral environment, should be fully understood. Historiographic techniques are needed to substantiate behavioral assumptions that have been introduced by geographers into their general models, particularly when these models are intended to explain long-term trends. If historians are to derive full measure from their efforts to interpret historical change, spatial environments as contexts for historical action should be considered. Geographic techniques of analysis focused on human spatial behavior in the past cannot help but add substantially to historical interpretation and analysis.

I am hesitant to suggest how interaction between the disciplines of academic history and geography might be cultivated. It is my conclusion, however, that the immediate concern should be the further definition of common interdisciplinary grounds. We should continue the debate as to how temporal and spatial parameters can be effectively related in historical and geographical research. How should we measure and analyze behavioral change through time and across space? On a less epistemological level focus should be placed on the historical actor and his awareness of alternatives of action in both time and space. How did persons in a past society perceive their place in the temporal and spatial sense, and undertake decision making accordingly? To what extent did the timing and spacing of persons, objects, and events in past environments influence the management of human affairs? In confronting these questions and the host of similar concerns implicit in the discussion above a stronger historical geography will certainly emerge.

Historical geography, viewed as a literature and as a discipline, has already matured considerably since the initial endeavors to interpret spatio-temporal patterns through the mechanisms of factual description. Turning to techniques of generalization, researchers sought first to organize their data through a variety of periodization and regionalization schemes, and then to construct simulation models to approximate changes in earth space. The theme of research currently being emphasized, however, seeks to bring this latter activity into sharper focus through direct consideration of past behavioral environments both at the broad level of environmental management and at the more restricted plain of spatial behavior. Focus on the mechanisms of human spacing, developing in human geography and in other disciplines, promises much for the future growth of historical geography. What role will historians play?

Gandhi: A Psychoanalytic View

Reviews by

JOAN V. BONDURANT and MARGARET W. FISHER

J. D. SUTHERLAND

ERIK H. ERIKSON. *Gandhi's Truth: On the Origins of Militant Nonviolence*. New York: W. W. Norton and Company, 1969. Pp. 474. \$10.00.

WHEN ERIK ERIKSON began this study he had not expected to rediscover psychoanalysis in terms of those elements essential to Gandhian *satyagraha*: truth, self-suffering, and nonviolence. This is what he tells us toward the very end of the book that he organizes around the strike of mill workers led by Gandhi in Ahmedabad during 1918. Erikson then discloses his hope that the reader will have come to see with him that he felt attracted to the Ahmedabad event not only because he had learned to know the scene and many of its principals and because it was time for him "to write about the responsibilities of middle age," but also because he sensed "an affinity between Gandhi's truth and the insights of modern psychology" (p. 440). His hope should surely be realized, for it is precisely this brilliant strand that delineates the most exciting (and distinctly Eriksonian) pattern as it is woven through a richly complex intellectual fabric.

The "truth methods" of psychoanalysis and of *satyagraha* do indeed represent "a convergence in human values which may well be of historical, if not evolutionary, significance" (p. 245). Erikson has so persuasively established this theme that other sensitive scholars, increasingly driven to grapple with contemporary challenges of human conflict, can, if they will, find here points of departure from which to advance toward elusive solutions for hitherto intractable problems.

Through these pages one observes the psychoanalyst extending himself by what he calls psychohistory to engage the first—and still the greatest—*satyagrahi*. In the meeting of two such remarkable practitioners, each an activist in his own right and both profoundly experienced in the confronting and resolving of conflict, the potential for further refinement and application of conflict-resolving techniques begins to unfold. Before returning to the seductive speculation that lends hope to the possibility

that techniques of creative conflict may yet be fashioned through further constructive probing of the two truth methods, let us turn to several other considerations that engage the reader as he follows the story developed in these exciting pages.

Erikson conducts his excursion into Gandhi's past both through the conventional recounting of historical event and by means of a clinical approach applied to life history. He has taken care to spell out the nature and limitations of his method of inquiry. The reader must ever bear in mind that a psychohistorical study is not to be taken as the presentation of a case history. All too often Western interpreters of Gandhi, with only a superficial understanding of Freud's insights and little more than a fragmentary knowledge of the psychoanalytic process, have found in Gandhi's candid written explorations of his feelings (especially those of guilt and puritanism) explanations for his remarkable behavior and extraordinary achievements. They have tended to engage in a reductionism to explain—and explain away—the greatness of the man and the import of his contributions. It has then been a short step to the oversimplifying of Gandhian techniques and, on occasion, even to the discrediting of the Gandhian approach as having no relevance for situations of conflict outside the Indian, or even Hindu, setting. *Gandhi's Truth* authoritatively demonstrates the dangers of such pitfalls. Erikson, whose considerable contributions to an understanding of the psychological foundations of great leadership are widely acknowledged, has cleared away some of these serious stumbling blocks. It is perhaps not too much to hope that we shall henceforth be spared further simplistic exercises in "originology."

At the same time that Erikson brings fresh insights into so difficult a subject, freeing us from many of the more disabling interpretations of the Gandhian method, he may also have left a residue of misunderstanding for those who would look to this book primarily to know the essential Gandhi. The Indian, or the specialist whose professional commitment is to the impossibly demanding understanding of India, must frankly raise a few questions about the manner in which Erikson deals with the cultural setting that conditioned Gandhi, just as he must also try to put into perspective the Ahmedabad *satyagraha* that Erikson takes to be "the Event."

We begin with a look at Gandhi's autobiography, begun in 1924 and published in book form in 1927, the work most often quoted by Erikson and the passages of which provide so much of the material for his psychohistorical interpretations. Erikson himself was well aware of a number of difficulties arising from the use of Gandhi's autobiography. There is the question of why indeed it was that Gandhi ever undertook the writing of an autobiography inasmuch as to do so does not appear to be congenial to the Indian temperament. As is too often forgotten, even the most famous of Indian autobiographies, that of Jawaharlal Nehru, was undertaken primarily to clear its author's troubled mind as he wrestled,

in prison, with urgent problems centered upon whether he could continue to accept Gandhi's leadership with all its baffling twists and turns. But beyond the rarity of autobiographical expression among Indians of Gandhi's time was a telling objection advanced by a friend and included in the introduction to his autobiography: "Supposing you reject tomorrow the things you hold as principles today, or supposing you revise in the future your plans of today, is it not likely that the men who shape their conduct on the authority of your word, spoken or written, may be misled?" Gandhi, who already had a history of disconcerting reversals of opinion in the course of his dedicated search for "truth," had to acknowledge the force of this argument. It did not, however, dissuade him. He was not proposing to write a "real autobiography," he said, but would recount his numerous "experiments with truth" for their moral or spiritual value, endowing these experiments with no claims to finality or infallibility, but rather turning the harsh light of truth upon his own shortcomings. So harsh indeed was Gandhi's self-appraisal that the reader is now and again made uncomfortable by what seems an overextreme false modesty. The severity of some of these self-accusations is the more surprising in that there is nothing in the self-confident tone of his earlier (and in part autobiographical) work, *Satyagraha in South Africa* (begun in 1923 and published in book form in 1927) to prepare the reader for the underlying self-abasement that surfaces so disconcertingly in the autobiography but is not characteristic of his other writings. Perhaps it is to be explained in part, at least, by the acute embarrassment he suffered over the laudatory biography written about him by Romain Rolland, the French pacifist and man of letters. The autobiography, like *Satyagraha in South Africa*, was written in weekly installments, and Rolland's book came to his attention just before the last chapter of *Satyagraha in South Africa* was published and about a month before the first installment of the autobiography appeared.

Erikson has raised questions concerning the adequacy of the English translation of the autobiography, saying that it has submerged the "subdued passion, the significant poignancy, and the gentle humor" characteristic of Gandhi (p. 60). He blames Gandhi's personal secretary, Mahadev Desai, for this. But although Mahadev is indeed listed as the translator of the Gujarati in which Gandhi wrote, his translation was handed over to Mirabehn (Madeleine Slade) for revision and the final choice of words was hers. The apportionment of blame may not always be certain as among Gandhi's mood, Mahadev's translation, and Mirabehn's revisions.

Erikson's case for considering the Ahmedabad strike to be the Event is something less than completely convincing, but a difference of opinion on this point is of little moment inasmuch as every Gandhian *satyagraha* would repay the deep probing that Erikson has devoted to this one. Indeed, it might well be that no final determination as to which was the truly great Event could fairly be made until they all had been studied. (Would

that Erik Erikson might decide to draw upon the insights he has already gained into the Gujarat scene to probe the baffling Rajkot *satyagraha*!)

It is noteworthy that Erikson was alerted to the potential importance of the Ahmedabad Event by Gandhi's chapter, "A Peep into the Ashram," which interrupted the narrative of the strike against the mill owners. "I came to suspect," he writes, "that that strike and that fast represented a demonstrable crisis in the middle age of a great man and was worthy of study as such" (p. 47). For one not sensitized to the psychoanalytic method, this "interruption" might well carry little significance. The ashram was being moved to a new site on the bank of the Sabarmati to escape an outbreak of plague. The strike was still on, but was making little progress. Gandhi had just previously noted his daily meetings with strikers on the river bank. In a narrative written in weekly installments it could easily be that the troubles connected with moving his band of more than forty people, including children, to a site on waste ground infested with snakes might quite realistically be judged the week's most noteworthy event. That the juxtaposition of chapters here may mean that "snakes" stand for mill owners and may therefore suggest "a breakthrough of Gandhi's anger against the mill owners—an anger which he had expressly forbidden to himself as well as to the striking and starving workmen" (p. 421) might, of course, be valid. But Gandhi's feelings toward both snakes and his chief antagonist among the mill owners were complex. He sided with the latter's dangerously unpopular decision to kill a band of rabid dogs, a matter that seriously tested his personal hierarchy of values.

Situations arose with considerable frequency in Gandhi's life in which the categorical prohibition against the taking of life, although it had been a strong element in his early cultural heritage, nevertheless gave way to the more truly compassionate position he had learned to appreciate in England, leading him to place a higher value on the prevention of suffering than on a more mechanical avoidance of "sin." His greatest ambivalence appears to have arisen over snakes. Some allowance should probably be made concerning fear of snakes in India, since venomous reptiles are more numerous, more deadly, and in closer contact with human habitations there than in Europe or North America. But Gandhi's fear of snakes, which he had dreaded since his early childhood, was greater than that of many Indians, and certainly greater than his pride could readily endure. He worked strenuously throughout his life to surmount this fear but at the time of the autobiography had to confess failure. In 1934, however, then in his middle sixties, he had so far succeeded that he was able to sit motionless while a snake charmer's tame snake coiled itself around his neck.¹

Gandhi's attitudes toward snakes and the killing of animals can be taken as illustrative of the many changes that took place in him as he developed

¹ Madeleine Slade, *The Spirit's Pilgrimage* (New York, 1960), 193.

from childhood to full maturity. He was subjected to strong Western cultural influences, and he freely adopted those Western ways that appealed to him. He also worked hard, and on the whole successfully, to rid himself of those weaknesses derived from his Indian childhood that he thought unworthy of the man he aspired to be. Perhaps the most important benefit that he gained from his immersion in Western culture was a freedom to pick and choose widely along the entire range of cultural alternatives that proliferate in India. No single guru, no exclusive religious beliefs, no caste restrictions were able to bind his freedom with their customary fetters. The restrictions that he did impose upon himself were in those areas where he felt himself most vulnerable to the dissipation of his powers through indulgence of the flesh—in particular the sexual drive and “the pleasures of the palate.” The “powers” that accrued to him through iron self-discipline in these areas were promised by age-old religious traditions of both East and West. For the rest, it may be enough to say that the category “abnormal” has less significance in the Indian context.

There are in fact few known forms of human behavior that do not come within the bounds of normality in India. Indeed one wonders if Western minds are sufficiently sophisticated as yet to deal adequately with the ramifications and affects of Indian culture. A child growing up in India can daily observe representations of the gods themselves engaging in behavior that might seem odd to a Western observer. This Indian child might also be familiar with Shiva Ardhanari—Shiva as Half-Woman—unitary, and yet with a lengthwise division into male and female. The mingling of masculine and feminine qualities in the divine hermaphrodite symbolizes not freakishness but perfection. The cultivation of “womanly” traits by Indian men is not looked upon as detracting from manliness. Neither is there anything at all unusual in a man’s “nursing” his father or any other older friend or relative. Many of those who make much of Gandhi’s nursing his ailing father appear not to realize that it was his uncle, rather than any female nurse, who replaced him at his father’s bedside. Erikson does mention the presence of the uncle, in passing, but the cultural significance of this revealing fact appears to have eluded him.

As Erikson grapples with problems of symbol, applying his knowledge of developmental stages and identity crises to the life of Gandhi, he pulls himself up short to interpose, midway through his book, a remarkable chapter entitled “A Personal Word.” Toward its beginning he explains that his justification for addressing Gandhi as though he were alive “would have been the conviction that psychoanalytic insights happen to complement your kind of truth by a strange reversal of the traditional roles of East and West,” for, he explains, “you are now a model of activism in our culture, while Western thought has provided a new technique of introspection” (p. 229). He has, by now, concluded that Gandhi had first to accept himself as an Indian, and one close to the masses as well, before

he could establish his identity as “no less than that of universal man” (p. 231). Erikson then proceeds to bring together what he has learned from Freud and what he has learned from Gandhi in a manner at once creative and compelling. Those who may have had the rare opportunity to experience the results both of *satyagraha* and of psychoanalysis can readily agree with Erikson that *satyagraha* and psychoanalysis are counterparts: “by dint of always being a self-analysis paired with an attempt to understand another man’s inner conflicts” (p. 244); because they both confront “the *inner enemy* nonviolently” (p. 244); because each “instrument of enlightenment” was forged “to include self-analysis, that is, the acceptance of himself as a person who shared [others’] inner mechanisms” (p. 245); “by replacing moral suppression with the belief that truth has enough force to make the patient [or the opponent] reveal what he had repressed” (pp. 245–46); “through the dictum that only as long as . . . nonviolent equality is maintained can truth emerge” (p. 246); by the rule that “disciplined self-suffering” is one of the tasks of both the analyst and the leader of *satyagraha* (p. 246).

The correspondence in method and the convergence in human values so clearly seen by Erikson and so lucidly set forth in his comparison of the two “truth methods” are summed up in the “main point”: “we are somehow joined in a universal ‘therapeutics,’ committed to the Hippocratic principle that one can test truth . . . only by action which avoids harm—or better, by action which maximizes mutuality and minimizes the violence caused by unilateral coercion or threat” (p. 247).

The student of India who has been caught up with what he has seen as the problem of self-abnegation (taken, at times, to mean the rejection of power!) must surely be struck by Erikson’s point that through the Gandhian method self-abnegation becomes “self-affirmation and a tool of truth rather than a weapon of revenge.” For one who has witnessed this on the field of conflict where *satyagraha* has come into full play, there is no difficulty in corroborating Erikson’s assertion, arising out of a quite different context, that such “phenomena occur quite wordlessly and unself-consciously in the ethicality of everyday life.” Then one knows, he explains, “how much of what we used to ascribe to the Devil’s wiles or to the id’s inexorable demands can be tolerated, if absorbed by love rather than negated by violent moralism” (p. 249).

In restating his most significant point Erikson reaffirms that nonviolence, “inward and outward, can become a true force only where ethics replaces moralism.” He then spells out what ethics means, in contradistinction to moralism. Ethics is marked, he says, “by an insightful assent to human values, whereas moralism is blind obedience; and ethics is transmitted with informed persuasion, rather than enforced with absolute interdicts. Whether the increasing multitudes of men can ever develop and transmit such an ethical attitude I do not know; but I do know that we are com-

mitted to it, and that the young are waiting for our support in attempting it" (p. 251).

Here, then, is the grand challenge. Erik Erikson has built the bridge for us between Freud and Gandhi—or, more precisely, between psychoanalysis and *satyagraha*. Only a man of genuine profundity, skill, and compassion could have successfully bridged such a chasm. As the clash of conflict grows ever more complex, the need becomes increasingly urgent for those who possess a like courage and vision to take up this challenge, fashioning for the purpose a technique adequate to our times. Surely it is no less than this of which Erikson writes when he describes as historical and evolutionary that confrontation of human values essential both to psychoanalysis and *satyagraha*.

JOAN V. BONDURANT
and MARGARET W. FISHER

WITH *Young Man Luther* Erik Erikson established a new kind of study of the great historical figure. In it he combined the insights of the psychoanalyst with an appraisal of relevant historical factors to illuminate how the mind of the great man made him capable of leading his contemporaries from a major impasse.

Early psychoanalytical studies of creative men have been criticized as reductionist. That certain unconscious fantasies might have played a fundamental part in what was created, both in determining its nature and in providing the relentless drive of genius, was not denied. What was seldom traced were the links between fantasies that were not unique in their pattern to the great man and the specific processes whereby he fashioned his innovative thought and action from these primal motives and his later perceptions of his own and others' needs. On this occasion Erikson again emphasizes that he is concerned with this larger task. For him the great man early in life evolves a special mission, a direction that must lead to all or to nothing. Such a structuring within his personality remains, however, a "developmental probability" whose subsequent realization is conditioned by an enormous range of influences from the social environment during the successive phases of development. The questions to be considered, then, embrace at least the following. What gives the developmental probability of the great man its specific content? That is to say, we want to understand how the essentials of his mature mission were shaped from his experience within his early family and social milieu. Allied with this question is that of the source of the necessary drive if his mission is to be transmuted into major social action. A third question takes us into his later experience. How does he, on the basis of his compelling potential, fuse his inner needs into an instrument that provides for others a way they intuitively sense as realizing what they have been unable to articulate and activate?

To appraise the theories offered about the origins of Gandhi's achievement we have to look at its nature. Here we note that Erikson is not making a comprehensive study of Gandhi's political life but is concerned specifically with his "Truth," that is, the political tool he developed—*satyagraha*, or militant nonviolence as it has become known. Erikson is fully aware of the varying evaluations of Gandhi's activities for India and of the blood bath that flowed from the hatred constantly simmering between the Hindus and the Muslims. It is poignantly ironical that the last serious eruption from this chronic source occurred in Ahmedabad itself, the town in which Gandhi finally tempered his political instrument. Such occurrences do not remove the value of Gandhi's Truth; they are among the issues that it was evolved to tackle. A comparison of Gandhi's instrument and Freud's method is one that Erikson makes, and we do not ignore Freud's achievement because of the limitations of the psychoanalytic method. *Satyagraha* as used by Gandhi on occasion clearly carried inconsistent restrictions stemming from conflicts in his personality, which he managed not by understanding but by severe moralistic prohibitions. We can assume, however, with Erikson, and with Romain Rolland, whom he quotes, the greatness of a method that "stirred three hundred million people to revolt" and that "introduced into human politics the strongest religious impetus of the last two hundred years."

Gandhi's Truth as a method of conflict resolution involved various steps, some of which, though now taken for granted, were revolutionary half a century ago—for example, obtaining the full study of the facts including those factors that influence public opinion and making a genuine attempt at arbitration with the clear announcement of actions to be taken. It is over the meaning of the Truth for the contenders that Gandhi's views have provided a disturbing challenge. For him the clash between the inevitable relativities of the truth as discerned by each party would only be overcome by a truth that transcended both. In reaching such a truth Gandhi's assumption was that no harm was to be done. Erikson suggests convincingly that in translating *ahimsa* we have to accept Gandhi's meaning as no violation to the essence of the other person. Without such a respect for the other's truth we merely get violence and counterviolence. In the situation for which arbitration produces no acceptable solution, the test of truth in the campaigners is the conviction that they will suffer for their goal to the point of death, while holding to the code of nonviolence. A *satyagraha* campaign therefore has to convince its participants of these requirements, and it was Gandhi's extraordinary charisma that he did this on such a scale. For him it was the leader's task to decide on the "true" course, and here we can, with Erikson, allow for the rather special historical condition that determined Gandhi's individual power in reaching such decisions and the cultural features that gave such significance to actions like fasting. As Erikson points out, if truth is actuality it

has to be achieved in each situation by fresh action and never by a mere repetition of ritualized acts.

The aspect of Gandhi's method that has aroused the most doubt is his belief, at least for himself, that its critical use demanded the abandonment of sexuality. It is here that an irrational manifestation of personal conflicts has taken over. His whole attitude to sexuality from early manhood was one of compulsive moralism against its "badness." It was a drain on the sources of the powers that man needs if he is to reach more mature ways. Erikson indicates that while Gandhi's personal choice in this respect does no justice at all to the role of sexuality in loving relationships, he was pointing to our need to understand more of what primitive phallic sexuality contributes to man's propensity to violence. For Erikson, Gandhi intuitively grasped that in situations of serious conflict each party must extend the boundaries of his self-feeling to include that of the other's. This was the essence of his drive and of his success. His capacity to speak for others down to the pariahs of his culture must spring from the fact that so much of his thought and action drew upon the deep and universal affects of mankind. What his failures have brought home is not the presence of basic error but his (and still our) lack of knowledge by which he could obviate the most urgent problems posed for mankind. It is here that Gandhi's method needs what psychoanalysis might help to supply. Erikson has been one of the most notable psychoanalytic thinkers—at times almost getting himself regarded as deviationist—trying to isolate at least some of the questions we have to study in regard to human violence, if not providing the answers. He likens Gandhi's contribution, admittedly with acknowledgment of the speculative flight, to an attempt in man to evolve a method of ritualizing aggression comparable with the instinctive inhibitors in most species. He has made it very clear we cannot talk of aggression in the hydraulic metaphors of the nineteenth century. Neither can we account for violence with the conventional notions of instinct. Erikson, more than any other psychoanalyst, has stressed the need to understand what we mean by such terms as "identity" and "the self," for it is in these areas that violence is released. (Arthur Koestler has stressed that for many centuries wars have been ideological, not basically economic.) For Erikson it is the emergence in man of what he calls a "pseudo-species" mentality that is the lethal product. The ethologists have shown us how intraspecies rituals have allowed species to survive their aggressive potential. The creation of human identity, a richly plastic potential for increasing adaptive powers, has been very largely "pseudo" in the sense that it neither embraces the species nor gives stability to any section of man. Its unstable integration needs others into whom are projected the inner forces that cannot be managed. In such a situation only destruction or subjugation of the threatening others is felt to be the safe solution. Gandhi's mission was to create a method by which such impasses might be overcome,

namely, by what Erikson puts as the “anticipatory development of more inclusive identities.”

Though Gandhi studied the New Testament, his Truth was always intended to keep political action and religion inseparable. What was felt to be right had to be so central to man’s sense of his self that he would gladly die for it. For him as in Christianity martyrdom included concern for the aggressor, but it was not to be invoked until all conceivable attempts had been made to replace the situation wherein “they know not what they do” by one in which both sides might see more clearly all the actualities—political, economic, and personal. By the time he ends his book Erikson confirms more than he expected his original intuition that Gandhi’s Truth had much in common with the thoughts of psychoanalysis. What emerges is that Gandhi’s method cannot be expected to solve conflicts that are the end product of too long-established pseudoidentities in man. Gandhi himself was notoriously unable to practice his method within his own family and at times within his ashrams, even when allowance is made for the fact that he was in middle age before he fashioned it. The aim of his method and much of its means are a legacy we cannot afford to ignore. Modern dynamic psychology—itsself developed from a method in which a nonviolent endeavor to understand the other replaced a moralistic assault—is perhaps not so much required in the situations for which Gandhi sought a creative solution as for preventing the growth to the position in human identity formation where anxiety and fear preclude the capacity to enlarge the self’s boundaries to admit the validity of the other.

Whatever value is given to Gandhi’s Truth today, we cannot but admire and wish to understand what made him a great man of his age. Two questions are posed on this issue: what were the origins of his particular mission, and what gave it its consuming energy? In answering the first the speculative possibilities are endless. Great men, however, seem to convey a sense of their destiny so that data about their early characteristics can be assembled. Oedipal conflicts, the favorite resort in the past, can readily be detected, but psychoanalysis itself is passing beyond the stage where these conflicts are regarded as fundamentals to one in which they are seen as outcomes of much that preceded them. Erikson recognizes these early influences but does not make use of the extent to which primary structuring of the person by internalized object relationships has been described by those stimulated in varying degrees by the work of Melanie Klein. Thus much of what dominated Gandhi’s mission incorporates a strong drive to make reparation. A difficulty for the Western analyst in speculating about the fantasy systems that Gandhi developed undoubtedly arises from the special setting of the Indian child in the extended family, with its compressed world of parental relationships complicated by the wider network in which they exist, the many children in the immediate environment, and the constantly impinging intensity of so many relationships with all

degrees of intimacy in the closely packed life space. Nevertheless, Gandhi's fantasies of being the powerful father along with his reparative devotion to his father are prominent. The Oedipal fantasies of Western culture are not ordinarily confronted with a father thrusting sexuality upon the boy in early adolescence, and Gandhi resented his arranged marriage most of his subsequent life. The closeness of his parents and the fact that he enjoyed an intensely affectionate relationship with them, as they did with each other, appears to have produced in Gandhi a most pronounced identification with the united and idealized good parents. But their relationship was to be freed from what he must have fantasied as dangerous sexual relations. As Erikson notes, no great man has so explicitly avowed his wish to be a good mother to others. Nor is this striking component of his personality associated with the giving up of his masculine self. In all the challenges he accepted he displayed all the courage and toughness of any male leader. A complex personality such as Gandhi's—and his inconsistencies in action, in his relationships, and in his moods were ever prominent—is obviously structured by a great range of fantasy systems. Erikson makes a good case, however, for giving a central role to this theme of keeping the harmony of the parental relationship in the evolution of Gandhi's personality, in its earliest phases and in later life when he arrived at his method in Ahmedabad.

Erikson recognizes that place he gives to what he calls "the Event" in Gandhi's life is likely to arouse skepticism. Gandhi's disavowal in his autobiography of its importance is rebutted readily enough, for every psychoanalyst is all too familiar with the defensive processes that can belittle critical events. What may be asked by the reader is whether the fascination exerted by the Sarabhai family on the author has influenced his judgment unduly. Gandhi was certainly arrested by the situation in which a brother and sister, devoted to each other, were the protagonists in the struggle. Equally, there is no doubt that Erikson's experience of being in the ambience of this family, now greatly extended, and of being able to talk to each of them in the place where the drama had occurred, might have stimulated his rich imagination to exaggerate the Ahmedabad happenings. (As one who has shared this experience in Ahmedabad, I have no hesitation in raising these possible effects!) Erikson's account to my mind survives this criticism. The Event was a deeply formative experience for Gandhi, and the evidence adduced in spite of all the difficulties carries conviction.

Our second question is one that Erikson does not answer, and no one else could in the light of our present knowledge. Erikson's probings, however, are surely pointing the way to where the answers lie. For what he is bringing out is that certain early experiences can create fantasy relationship systems that give an intensity of feeling to the self and that establish a constant inner pressure to reproduce their patterns within a constantly

adaptive actualization. Cybernetic models will no doubt fill out from the data of the neuropsychological substrates of affective experience as well as its psychology what we cannot do at present, that is, account for the compelling intensity of certain emotional systems when they are combined.

If we keep in mind his limited objective, Erikson has produced a great man's contribution. His preoccupation with mankind's dilemma of being faced with primitive emotional forces over which we have little control and that are able to express themselves with annihilatory power has certainly drawn him to look deeply into this great historical figure. Much of what could be debated remains marginal to his theme of the nature and origins of Gandhi's method. On these he brings his rich and mature knowledge to bear with the result that we have a study that is seminal in the innumerable considerations it raises on the urgent issues of managing our irrationality.

J. D. SUTHERLAND

Sir Frank Stenton (1880–1967)

A Review Article by V. H. GALBRAITH

DORIS MARY STENTON, editor. *Preparatory to Anglo-Saxon England: Being the Collected Papers of Frank Merry Stenton*. New York: Oxford University Press. 1970. Pp. xiv, 425. \$14.00.

F. M. STENTON. *Anglo-Saxon England*. (The Oxford History of England, Volume 2.) 3d ed.; New York: Oxford University Press. 1971. Pp. xli, 765. \$12.75.

DORIS MARY STENTON. "Frank Merry Stenton." *Proceedings of the British Academy*, 1968, 54 (1970): 315–423.

THE PUBLICATION BY LADY STENTON of *Preparatory to Anglo-Saxon England: Being the Collected Papers of Frank Merry Stenton* provides the opportunity to take stock of the career of a distinguished historian whose name will forever be associated with the study of Anglo-Saxon history and with the University of Reading. Born on May 17, 1880, an only son, in a God-fearing home, Frank Stenton was far too delicate a child to go to school. Instead he had private tutors and showed great promise as a musician. At seventeen chance led him to the "nascent agriculture department at Reading," then no more than an extension college. This accident determined his whole career; for though he went on to Oxford and took a first class in the History School, it was a research fellowship at Reading that finally rescued him from school teaching. In Reading he spent the rest of his long life, becoming virtually one of the founders of the university there. From 1926 to 1946 he served as professor; from 1946 to 1950 he was vice-chancellor; and in 1948 he received a knighthood. By this time he was a dominating figure in the small world of medieval historical research, and during his long retirement (1950–67) he exercised a still wider influence in connection with the Institute of Historical Research and the government-sponsored *History of Parliament*.

The value of Lady Stenton's exemplary volume for the study of Anglo-Saxon England is not easily exaggerated. It both rounds off the bibliography of Stenton's published writings, already set out in Sir Christopher Hatton's *Book of Seals* (1950), which was presented to Stenton on his seventieth birthday, and it republishes a generous selection of his periodical publica-

tions. But, most fortunately, it does not stand alone. The dry bones of bibliography are now made to live by Lady Stenton's full-scale obituary notice in the *Proceedings of the British Academy*. The memoirs of widows upon their husbands too often smack of hagiography, but no such charge can be brought against this minute narrative of a scholar's life. From first to last it is written, if not with detachment—that would be impossible—yet with studied objectivity. What would we not give to possess this wealth of detail about Bishop Stubbs, or F. W. Maitland, or T. F. Tout? The life of no other scholar of equal eminence is now so completely documented; and Lady Stenton's unflagging restraint has produced a portrait so authentic that her narrative cannot be read without emotion by all who knew her husband. One is tempted to add that the two documents, taken together, chronicle the most vital and progressive epoch of research upon pre-Conquest England since J. M. Kemble's publication of the *Codex Diplomaticus* (1839–48).

Stenton came to manhood at a time that, in retrospect, appears as the golden age of historical writing on medieval England. By the beginning of the present century J. H. Round, Sir Paul Vinogradoff, Maitland, Alfred Plummer, and W. H. Stevenson had all published work "directly related to the main substance of history."¹ Much of it was fiercely controversial, attacking the conclusions of the mid-Victorians like Stubbs, E. R. Freeman, J. R. Green, F. W. Seebohm, and others.

From all this work there emerged a conception of Old English history which was consistent in itself, and in keeping with the political thought of the time. In origin and in essential features, which were discernible to the end, Anglo-Saxon England was a land of village communities composed of peasants who were free and responsible members of society.²

The "Germanists," in short, had routed the "Romanists," whose chief protagonist had been Seebohm, and to the youthful Stenton it seemed that the time was ripe to rewrite Anglo-Saxon history, embodying and developing the conclusions of these new master historians. The result is concisely stated by Lady Stenton in the opening words of her preface to *Preparatory to Anglo-Saxon England*.

The appearance of *Anglo-Saxon England* in 1943 was a landmark in English historiography. It marked the end of a long task. Up till then my husband had spent his life largely preparing for and then writing this book, which was firmly based on every available type of evidence about the Anglo-Saxons. Place-names, coins and charters, wills and pleas, archaeology and the laws of the Anglo-Saxons were all for the first time adequately used to produce a balanced narrative, supported by Domesday Book and the twelfth century charters which made it easier to understand the earlier material.

¹ For Stenton's own summary of earlier research, written three years after the publication of *Anglo-Saxon England* in 1943, see *Preparatory to Anglo-Saxon England*, 346–56.

² *Ibid.*, 349.

The success of *Anglo-Saxon England* was immediate, and it has ever since been a best seller, superseding all earlier histories of the Anglo-Saxons. Dr. Philip Grierson in the *English Historical Review* described it as "one of the most valuable contributions ever made to our knowledge of the history of our own land."³

Such is Lady Stenton's broad assessment of her husband's total achievement; and it explains the title given to his *Collected Papers*, which might otherwise puzzle the uninitiated reader. For the successive stages in the realization of Stenton's lifelong ambition we have the detailed evidence of the British Academy biography, which minutely traces each successive step. No useful purpose would be served by attempting to summarize it here, since its whole value lies in its precise record of events. It has, however, wider implications as the best factual account of the gradual take-over of historical research by the universities during the last half century.

At the close of the nineteenth century Oxford and Cambridge, whose social prestige far exceeded that of all other British universities, made but slight provision for, and gave less encouragement to, the training of post-graduate students in the process of original research such as was given in France, Germany, and the United States. The main object of Oxford and Cambridge was to prepare well-to-do young men, recruited from the Public Schools, for entry into the public service, the professions, and the Church; and the dons, uninfluenced, for the most part, by the increasing emphasis laid elsewhere upon further discovery, were generally content to refer their pupils to Stubbs's *Constitutional History* (1874-78) as the last word on the evolution of medieval institutions. But already in England there was a growing demand from young scholars for a more professional training. The leading figure in this movement was Professor Tout (1855-1929), who, though he had taken a good first in history as a Balliol undergraduate, had to spend some years teaching at Lampeter before he was made professor at Manchester in 1890. There he developed a new approach to the B.A. degree based on the principle that the training best suited to a professional historian was also the best for the ordinary history graduate. Five years later the young Stenton, "never one to write to the papers," notes Lady Stenton, wrote a remarkable letter to the *Manchester Guardian* that pinpointed the situation. "The essential point," he wrote, was

that research as such finds no place in the Oxford scheme of historical education. Historical research at Oxford is a luxury both to the undergraduate, who can only apply himself to the technicalities of this art at the imminent risk of losing a class in his final examination, and to the graduate, who will normally find no endowment from university or college sources available for the purpose of his studies.⁴

³ Grierson, review of *Anglo-Saxon England*, in the *English Historical Review*, 60 (1945): 247-49.

⁴ Quoted in "Frank Merry Stenton," 347.

This put the problem in a nutshell: research was a luxury open only to the rich, and from the moment of his graduation at Oxford Stenton had to harness his ardor for research to some form of gainful employment. This was provided by the *Victoria History of the Counties of England*, an ambitious enterprise begun by Doubleday and William Page at the turn of the century. Each county was to be provided with a translation of the text of the Domesday Book, together with an elaborate historical commentary. Of this section of the work J. H. Round was inevitably the high priest, he himself dealing with the East Anglian counties. In June 1902, we learn from Lady Stenton, Frank wrote to his mother that

Doubleday had the great man Round to see me and they want me to begin at once on Nottinghamshire history, receiving payment for each piece done. . . . The first step is a translation of Domesday Book as it relates to the county. Of this they want me to do a specimen page—if Round thinks that satisfactory which Doubleday says is practically certain . . . the payments will come to about £20–25 for the Domesday translation alone.⁵

So began Stenton's lifelong connection with the *Victoria County History*, for which he wrote the introductions to the text of Derbyshire (1905), Nottinghamshire (1906), Leicestershire (1907), Rutland (1908), and Huntingdonshire (1926); and in 1924 he wrote an introduction to Lincolnshire for the Lincoln Record Society. The meeting with Round in 1902 was to prove crucial for Stenton for it made him into Round's greatest disciple. "All his life," Lady Stenton writes, "Frank remembered Round's kindness with gratitude," never forgetting "the top hat and frock coat in which Round was clad."⁶ On these famous introductions county historians still rely, and upon them Stenton's later output was based. His minute acquaintance with the Domesday text underlay the final flowering of his genius in his *Anglo-Saxon England* (1943), when he was more than sixty years of age. No one, not even Maitland in his *Domesday Book and Beyond* (1897), had made such profitable use of that superb collection of statistics as a guide to the preceding centuries.

Important as the meeting with Round was to prove, the work with the *Victoria County History* was insufficient to provide a livelihood, and Stenton, despite his precocious brilliance, had to spend five years in teaching at Llandovery School in Wales (1908–12). Then, at last, the offer of a research fellowship at Reading—not yet a university—provided the *entrée* into academic life. It was only £100, but was nevertheless the turning point in his life and the realization of his ambition to become a professional historian. No wonder that he stayed at Reading for the rest of his life, and like Goldsmith's parson "nor e'er had chang'd, nor wished to change his place."

⁵ Quoted in *ibid.*, 348.

⁶ *Ibid.*, 349.

And so at the early age of thirty-two years, Stenton was a master of Domesday studies on which, thanks to Round's encouragement, he had already completed a decade of apprenticeship. I stress this point not to correct Lady Stenton, but to clear up a certain ambiguity implicit in the title given to Stenton's collected papers: *Preparatory to Anglo-Saxon England*.⁷ His *Anglo-Saxon England* still stands today, virtually unchallenged and streets ahead of other Old English histories—a period on which the leading scholars, like Professor Dorothy Whitelock, are his pupils. But it would be a great mistake to limit Stenton's historical achievement to pre-Conquest history. *Anglo-Saxon England* itself includes the best available account of William the Conqueror's reign. Nor is this reign a *terminus ad quem*, for Stenton's mastery extended a full century later. It was, in fact, his *Documents Illustrative of the Social and Economic History of the Danelaw* (1920) that first established his scholarly reputation;⁸ while his Ford Lectures, delivered in 1929, and published under the title of *The First Century of English Feudalism: 1066–1166* (1932), may well retain their historical authority for an even longer future than his Anglo-Saxon studies since they rest upon a close acquaintance with the more copious and more reliable evidence that becomes available after the Conquest. Both were written in the prime of life and have profoundly influenced more recent work. Much of the same mastery is already apparent, however, in Stenton's *Norman London* (1915), and even in his *Early History of the Abbey of Abingdon* (1913), a phenomenal little book in which he first bridged the gap between Old English and Norman history. "Frank's work," Lady Stenton notes, "was extraordinarily mature and finished."⁹ Even his *William the Conqueror and the Rule of the Normans* (Heroes of the Nations Series, 1908) was a fantastic achievement for a man still under thirty.

Thus, thanks to Lady Stenton's biography, in following Stenton's footsteps through a long life we have, in fact, been tracing the progress of medieval historical research in England through more than half a century. The lead first given by Tout at Manchester after 1890 soon made him the outstanding personality in the development of a new approach to the past until his death at the age of seventy-four in 1929. Thenceforward, Stenton, who like Tout had come up the hard way, more and more attained the same comparative distinction, together with Sir Maurice Powicke, who began as Tout's pupil at Manchester.¹⁰ In 1928 Powicke, who was born in the same year as Stenton, became Regius Professor at Oxford, and he lived until 1963. His life is closely associated with Oxford, which he

⁷ The ambiguity could be avoided by reprinting the title as *Preparatory to "Anglo-Saxon England."*

⁸ Lady Stenton describes this study, commonly known as *Danelaw Charters*, as built upon "early post-Conquest evidence." "Frank Merry Stenton," 362.

⁹ *Ibid.*, 354.

¹⁰ Powicke's career was strikingly parallel with that of Stenton, and attended by the same sort

brought into line with other British universities by developing the practice and machinery of postgraduate research. But from 1890, or even earlier, the systematic development of personal historical inquiry was overwhelmingly dependent upon individuals, men like Tout, Stenton, Hamilton Thompson at Leeds, and other provincial professors, who collectively bridged this strange gap in the history taught at Oxford and to a lesser degree at Cambridge.

There was, of course, an element of social snobbery inherent in the passive resistance that the Oxford establishment for so long offered to the new spirit; and though it has now very greatly expanded the apparatus of research by the Ph.D. degree, the undergraduate curriculum of the History School still retains its traditional outline. Yet, odd though it sounds, this Oxford conservatism has immensely stimulated the development of all the other British universities, which in recruiting their history teachers now demand printed performance as well as personality. A century ago medieval research was the hobby of well-to-do amateurs who were not formally connected with the universities. Now, by what amounts to a take-over, it is the prerogative or distinguishing mark of all our universities, and as such has proved a powerful academic leveler. The senior professors of the majority of British universities today are men formed on the model of the early pioneers: men who have somehow managed, despite the teaching load, to produce distinguished research. And no one who has visited these universities can fail to have noticed how greatly their example has revived the interest and excitement of their pupils.

Today, however, when there is more money available for historical study than there are people capable of doing it adequately, we should still remember with gratitude a small minority of great Oxford scholars who, during the gap mentioned above, brought to historical research a mastery of method equal to that of the best Continental scholars in France and Germany. Among these, three names are of crucial importance—Reginald Lane Poole (1857–1939), who for many years brilliantly edited the *English Historical Review*;¹¹ Sir Paul Vinogradoff, from Russia, who was professor of jurisprudence from 1903 to 1925,¹² and Sir Charles Firth, Regius Professor of History from 1904 to 1925, who, in Lady Stenton's words, "fought what seemed at times like a losing battle against college tutors who were uninterested in encouraging young men to get the train-

of difficulties. See his obituary, by R. W. Southern, in the *Proceedings of the British Academy*, 1964, 50 (1965): 275–304.

¹¹ University lecturer in diplomatic (1896–1927) and a pioneer of early administrative history, Poole exercised immense influence as editor of the *English Historical Review* (1901–20), whereby he became the personal friend and helper of almost all the serious young scholars, both men and women, of the next generation. See Sir George Clark's obituary of Poole in the *English Historical Review*, 55 (1940): 1–7.

¹² Vinogradoff introduced the Continental seminar to Oxford and published much fine work in the British Academy series of *Records of the Social and Economic History of England and Wales*.

ing necessary for the sound writing of history.”¹³ These, with others like H. W. C. Davis, were to prove the linkmen in this century-old transformation of historical study, for they were the friends and even the teachers of Stenton and many other devotees of research in the provincial universities, and they strove unceasingly to advance the careers of pupils sent to them by the professors *in partibus*, whose outlook they shared. They were, in fact, the true heirs of Bishop Stubbs himself, who had taught Firth and Tout as Oxford undergraduates and was never much in sympathy with the establishment over which he presided as professor from 1866 to 1884.

The overall impression of Sir Frank Stenton conveyed by Lady Stenton's two volumes of meticulous record is one of youthful precocity, all-round ability, and immense vitality despite his apparent physical frailty. As he could have succeeded in any profession by the sheer range of his intellectual interests, Lady Stenton's insistence upon his early decision to rewrite Anglo-Saxon history is of first importance. He could, had he wished, have made good as a musical composer, and he also showed aptitude as a youth for natural science. Yet by the time he had left Oxford he had already decided to become—not an archeologist, though his biographer confesses “I became a dedicated historian from the first lecture I attended on Roman Britain”;¹⁴ not a philologist, though he lived to edit with Allen Mawer the English Place-Name Society's publications; not a military expert or a civic historian despite remarkable contributions to both of these then rising subjects; not even a numismatist though few men knew more about early coinage—but a professional historian, setting out to erase “the thin red line of the Norman Conquest” by joining up the researches of Maitland and Round with the dark hinterland of Old English history. Nor did this early ambition limit his more practical activities. He was an outstanding vice-chancellor of Reading University, but he was also a distinguished teacher “who never gave the same lecture twice, and never wrote them out in full.”¹⁵ Yet on such formal occasions as the Ford Lectures he held his crowded audience for six weeks by sheer oratory, and he was unequalled upon committees. His prose, too, had its own distinctive rhythm; while in private life—at dinner—he was totally relaxed, extremely lively, and at times positively slangy. Soon after their marriage in 1919 the Stentons moved to Whitley Park Farm, Reading, where for a generation they dispensed a lavish hospitality to the innumerable scholars who sought help and advice.

The friendships arising from these informal social confrontations resulted,

¹³ “Frank Merry Stenton,” 347. This can easily be exaggerated, for Oxford's dons, owing to the peculiar “college system,” have for centuries delighted in apocryphal stories of dons being sunk in “port and prejudice,” and still do so. It is not, for example, a fact that some crusty life fellow, speaking of Tout's *Administrative History*, stated that in his opinion “the Great Seal might well have been better thrown into the grate, and the Privy Seal down the privy.” Nor was R. L. Poole descended from the “Pools of Siloam”!

¹⁴ *Ibid.*, 367.

¹⁵ *Ibid.*, 370.

not seldom, in the publication of ancient local records; for both the Stentons combined enthusiasm for old records with an unusual practical ability for getting them into print. Thus, the archives of Lincoln Cathedral now approaching completion in ten handsome volumes¹⁶ owe their publication to a casual meeting with Canon Foster half a century ago, while the Northamptonshire Record Society, a model of its kind, owes its very foundation to a meeting with Miss Joan Wake.¹⁷

All this and more is intimately conveyed by Lady Stenton, who in one place apologetically remarks: "It is difficult to keep myself out of the story since we did everything together."¹⁸ It was certainly true, but even so it is the greatest understatement in either book; and something, however little, must be added regarding the immense debt he owed to her. A historian of great achievement in her own right, her lasting contributions to research take up about the year 1166, that is to say where her husband's written works, though not his interests, ended, and continue well into the thirteenth century. When still his pupil at Reading, she has confessed to a somewhat languid interest regarding the *Danelaw Charters* in which her husband then was "bubbling over" with enthusiasm. Soon after their marriage she became the secretary of the Pipe Roll Society, whose publications—and they were very technical—she successfully directed for many years. From these she branched out on the Plea Rolls, and more particularly upon the reign of King John, in whose rehabilitation as an administrator she played a notable part. On the single occasion on which the Stentons visited America, in 1961, it was in order that she should address the American Philosophical Society. Together they formed an impressive and at times a formidable combination both in English learning and in social life, each *primus inter pares* in his own sphere, and mutually indebted. The scholarship of both is marked by a new and greater expertise in dealing with the sources of medieval history in the original manuscripts never fully attained in the nineteenth century; and these new standards are reflected in the publications of local history institutions, and most notably, in the Lincoln and Northamptonshire Record Societies. Either singly or together they have had a finger in nearly every historical pie over half a century. But even so, their overwhelming service to English historical research has lain in their personal encouragement of younger scholars whom they taught and later supported in the vital problem of professional advancement.

¹⁶ The *Registrum Antiquissimum*, published by the Lincoln Record Society, was commenced by Canon Foster and carried on after his death by Professor Kathleen Major.

¹⁷ See her admirable, warm obituary of Stenton in *Northamptonshire Past and Present*, 4 (1968-69): 181-84. "I remember," writes Miss Wake, "once complaining to him about the time consumed by administration." " 'Yes,' " he said, " 'what I call odd jobs!' "

¹⁸ "Frank Merry Stenton," 379.

Reviews of Books

GENERAL

G. H. R. PARKINSON, editor. *Georg Lukács: The Man, His Work and His Ideas*. New York: Random House. 1970. Pp. 254. \$7.95.

BERTHOLD P. RIESTERER. *Karl Löwith's View of History: A Critical Appraisal of Historicism*. The Hague: Martinus Nijhoff. 1969. Pp. 108.

The volume on Lukács is "based on" most of a series of lectures given at the University of Reading in 1968; we are told somewhat cryptically that "it has not proved possible" to include two of them. Of the volume as it stands, only parts are relevant for a joint review in the field of philosophy of history (certainly only parts of it lie within my area of professional competence). These are chiefly the editor's introduction, a sound and informative chronological sketch of Lukács' career, both intellectual and political, spiced with some analytical passages; the last two chapters, "Lukács' Views on how History Moulds Literature" by David Craig and "Lukács' Concept of the Beautiful" by Stanley Mitchell, one of the translators of Lukács' *The Historical Novel*; passages, particularly on that work, in A. G. Lehmann's "The Marxist as a Literary Critic" and, true to form, the sections on Dilthey in H. A. Hodges' "Lukács on Irrationalism." Some of the rest of the volume will be accessible and of interest only to those versed in the arcana of Marxist sectarianism. Of the parts under consideration here, the weakest, in my judgment, is unfortunately Craig's, potentially the most important for historians. The weakness is due not only to Craig's self-confessed and, I must say rather astonishing, limitation to those works of Lukács available in English but also to its diffuseness; many of his specific points, both about *The Historical Novel* and Sir Walter

Scott in particular and about the relationship among esthetics, literary criticism, and history in general are made better by Lehmann and Mitchell. Nevertheless, Craig's chapter is by no means without value and interest. In fact, all the relevant parts of the Lukács volume are illuminating, critical as well as expository, and above all clear, even where the subject is obscure.

The small monograph by Riesterer, a revised version of a Ph.D. thesis at Wayne State University, though for the most part professionally relevant to historians, could perhaps be better described as redundant. Riesterer has read all of Karl Löwith's voluminous and scattered output and summarizes, with sometimes unduly lengthy quotations, the major and some of the minor works. Though having studied for a year at Heidelberg while Löwith was active there, he is laudably unafraid to offer adverse criticism of Löwith's changing attitude to "historicism" and his ultimate ineffectual attempt to combat it. Since Löwith was motivated to undertake this attack, not because of any intrinsic shortcomings of "historicism," especially in Dilthey's version, but because he judged historicism to have issued in radical relativism and thence in the "decisionism" of Carl Schmitt that in turn gave support to National Socialism, the question arises whether Löwith was worth a monograph in the first place. As between two philosophers who, one way and another, have spent their lives wrestling with Hegel, Lukács is altogether the more powerful intellect, despite his acceptance of an orthodoxy whose damaging effects are shown throughout the volume on him, most trenchantly perhaps by Lehmann. Lukács' superiority to Löwith is suitably reflected in

the two books under review. The volume on Lukács, though by its nature almost inevitably a little patchy, is clearly the work of mature minds. Riesterer, by contrast, does not inspire confidence. Both his information and his argument are sometimes at fault, and doubt even creeps in whether he is always quite sure what he means by "historicism." The monograph is also marred by a number of editorial and technical defects.

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F. R. COWELL. *Values in Human Society: The Contributions of Pitirim A. Sorokin to Sociology*. (An Extending Horizons Book.) [Boston: Porter Sargent.] 1970. Pp. xii, 480. \$8.95.

Pitirim Sorokin died in 1968 at the age of seventy-nine, after an extraordinary personal and intellectual life. Born in a Russian peasant village, he engaged in revolutionary activities as a student and teacher at St. Petersburg and was jailed on several occasions by tsarist police. A friend of Kerensky, Sorokin became his secretary in the provisional government, and he was later imprisoned and condemned to death by the Bolsheviks. He was reprieved through the influence of young Bolsheviks who formerly had been his students, and he was allowed to establish a new department of sociology at Leningrad, where he taught until 1922, when he went into exile. Lectures in the United States led to an appointment at the University of Minnesota from 1924 to 1930. He then became the first professor and chairman of the new department of sociology at Harvard, where he remained until his retirement in 1959.

Sorokin was interested in a wide range of sociological topics and wrote three dozen books and over two hundred articles. He became most widely known for his ambitious attempt to formulate a theory concerning the rise and fall of unified cultural systems in the past. Initially presented in four volumes entitled *Social and Cultural Dynamics* (1937-41), Sorokin's philosophy of history appeared to be a prophecy of doom for the Western world. Contemporary Europe and America were in the declining phase of "sensate" culture, which was marked primarily by its hedonism and by a materialistic view of reality. Sorokin protested, however,

that he was not pessimistic concerning the future of Western civilization. The next phase was to be a newly invigorated "idealistic" culture combining the best of the "sensate" and those of the subsequent "ideational" culture, which would be defined by its view of reality as nonmaterial and eternal, and in which hedonism would be replaced by spirituality. Sorokin's prediction was based upon the cyclical pattern of the past, in which, for example, the predominantly "ideational" culture of the Middle Ages was followed by the synthesizing "idealistic" thirteenth century, which evolved into modern "sensate" culture.

Values in Human Society is a clear and accurate exposition, as well as an earnest defense, of Sorokin's theories, presented by a long-time admirer. F. R. Cowell is an amateur historian-sociologist-man of letters and a former English civil servant, now in his seventies. Cowell first published an introduction to Sorokin's philosophy of history twenty years ago. The present volume incorporates a revision of the earlier book and adds to it summaries of Sorokin's autobiographical writings, his other sociological scholarship, and a discussion of the reception of Sorokin's ideas.

Values in Human Society is the best available introduction to Sorokin's philosophy of history. For his work in general it may be supplemented by the collection of criticisms and Sorokin's responses edited by Philip J. Allen, *Pitirim A. Sorokin in Review* (1963).

ROBERT ALLEN SKOTHEIM
University of Colorado

GABRIEL JACKSON. *Historian's Quest*. New York: Alfred A. Knopf. 1969. Pp. viii, 234. \$6.95.

In *Historian's Quest* Gabriel Jackson recreates candidly and sincerely the context in which he wrote *The Spanish Republic and the Civil War* (1965). He describes both general historic problems that interest him and those in Spanish history (where he acknowledges the influence of Américo Castro). It is his account of interviews with participants or with bureaucrats who guard the archives, however, that provide the most insight into Jackson's historical perspective and methodology. Jackson scrupulously describes his personal and intellectual preparation for these interviews: friendship

with Spanish exiles in Mexico, literary preferences, graduate work in Toulouse where he met other Spanish exiles, dedication to civil egalitarianism, and lack of concern with formal religion. All this enabled him to communicate easily with liberal Spanish politicians and intellectuals who defended the constitution of 1931 and provided a vantage point for him to view the complex political spectrum, which ranged from those who would substantially or in part revise that constitution to social revolutionaries or reactionaries who opposed it outright.

Even granting immense good will, can any one individual comprehend each and all of these ideologies, complicated as they were by personal ambitions? Conflicting viewpoints obscure not only motivation but even the facts of a controversial event of which there is no documentary record. Consider the convent burnings of May 1931. Jackson interviewed Miguel Maura, the "Catholic" republican minister of the interior in 1931, whose account differed in detail and evaluation from that of Diego Martínez Barrio, a Radical minister in that same cabinet. Jackson has great respect for Martínez Barrio and favors his view. Yet Maura's impassioned but factual report (presented fully in his book, *Así Cayo Alfonso XIII* [1962]) is confirmed in many ways by a Socialist minister (Indalecio Prieto, in *Cartas a un Escultor* [1961]). The detail of the Socialists' vote in that crisis, which Jackson emphasizes in crediting Martínez Barrio's account, is described differently in Maura's book and in Jackson's account of his interview with Maura. Much of the confusion results from the larger conflict between Maura's view of the burnings as an organized challenge to authority justifying harsh action and the view of an anticlerical like Martínez Barrio that the burnings were a genuine expression of public anticlericalism. In *Historian's Quest* Professor Jackson provides a forthright account of the way he personally resolved these conflicting testimonies.

JOAN CONNELLY ULLMAN
University of Washington

ARNOLD TOYNBEE. *Cities on the Move*. New York: Oxford University Press. 1970. Pp. ix, 257. \$6.75.

Toynbee's latest book offers "to examine the present urban explosion in the light of the previous history of cities." Inspired by C. A. Doxiades, the city planner, and Jean Gottmann, the geographer, he applies his characteristic method to a subject of grave contemporary concern. Toynbee still impresses the reader by the immensity of time into which he sets his topic; by the show of learnedness (sometimes in danger of becoming an end in itself); and by the globalism of his perspectives. He still is the "complete historian" in achieving a close, if occasionally naive, fusion of himself with his history through autobiographical anecdote, recollection of firsthand impressions, or statement of personal opinion; in his unflagging moralism; and in his happy knowledge that history is part of the future and the future is part of history. Yet in his new interest he seems still part of an earlier generation.

He compares the vast daily movement of urban commuters with man's nomadic existence in the distant past, viewing the new nomadism as a result of the outflow of cities, an outflow devastating the countryside "more thoroughly and more lastingly than it had ever been devastated by any nomad horde." This terrifying trend has created a number of huge megalopolis and will lead, in the foreseeable future, to the rise of the global ecumenopolis. After discussing, in a somewhat playful and not always convincing manner, various types of cities, he concludes with a guesswork sketch of the coming World-City. He sees ecumenopolis as an interrelated network of giant megalopolis (the present ones and a new one in the area of the great lakes of East Africa). It is choked with slums and vehicular traffic; its air and water polluted. Toynbee is opposed to immediate slum clearance on the ground that we desperately need both existing and new housing. He berates the "sly collective selfishness" of suburbia and would abolish suburban self-government. He rails against the irrationality of mass transportation by private automobile and by airplanes—he prefers railroads. He also looks for the preservation of small communities the size of ancient Ur or eighteenth-century Weimar. Human happiness in the ecumenopolis must be achieved, he now says, by rearranging man's external en-

vironnement (as Doxiades argues) rather than by searching for a new epiphany, as he used to say.

The professionals of urban studies may find the underlying theory of urbanism somewhat simple for their purposes: the basic intellectual equipment and orientation of this book is essentially that of *The Study of History*. While Toynbee touches on technology, he slights science; and the modern cities of Europe are unduly underrepresented. In emotional tone this volume is curiously mixed of nostalgia for Ur-Weimar and the realization that "we cannot revert to the premechanization way of life"; of rejection of ecumenopolis as currently looming ahead and acceptance of its promise (including world federation). In general it would seem to belong more to the corpus of Toynbee's work than to that of urban studies as currently practiced.

THEODORE H. VON LAUE
Clark University

Villes de l'Europe méditerranéenne et de l'Europe occidentale du Moyen Âge au XIX^e siècle: Actes du colloque de Nice (27-28 mars 1969). (Annales de la Faculté des Lettres et Sciences humaines de Nice, Number 9-10, 3rd and 4th Quarter 1969. Centre de la Méditerranée moderne et contemporaine.) [Paris:] "Les Belles Lettres." 1969. Pp. 363.

These twenty-four papers were presented at a conference on urban history held in Nice in 1969. Scholars at every stage in their research came together from Italy, Belgium, France, and Spain. The results are uneven; some scholars merely repeat what they have published before, others are at such an early stage of their work that they do little more than describe their sources, and still others, notably H. Lapeyre, M. Bouloiseau, and E. Corvisier, present important contributions to European social history. None of the participants seems to have feared that the details of the municipal administration in some particular town, the response to a plague in another, the social structure, militias, and land ownership, would bore their listeners or readers. None reaches out to make a comparative analysis, and all avoid questions about the nature of urban society, the definition of towns, and so forth. Not one paper explicitly raises methodologi-

cal questions, and no general models for an urban history are presented. Not a single work by a sociologist, anthropologist, or ethnologist is cited.

The reason for this attention to the particular is perhaps obvious. In the last century scholars in the history of medieval and early modern towns have had a surfeit of theories about urban history, and as the heirs of Fustel de Coulanges, Henri Pirenne, and others, they tend to take for granted this theorizing or go beyond it. Through these papers runs a refrain expressing reservations about the older, more general conclusions about the unique role that the town played in Western civilization. Instead, we find that every element of the urban population was linked to rural society. Indeed, a general theme in these papers is that no aspect of urban history may profitably be studied without paying attention to its rural components.

There are references to the general questions being debated by historians, questions that specifically have little to do with urban history. For example, A. Marongiu stresses the feudal character of the relationship established between lords and the "collective signory" or town. F. Vercauteren assesses the factors leading to an economic "take-off" in twelfth-century Arras and notes how this stimulated the demand for special privileges on the part of the townsmen. E. Ruano sees the effect of the *Reconquista* on the development of towns beyond the Asturias region of Spain, while C. Battle describes how the depression and plague at the end of the fourteenth century led to an attempt to "democratize" the municipal government of Barcelona.

Much more subtly, N. Coulet suggests, on the basis of a study of "lost villages" around Aix in the fourteenth century, that the village itself might well have been abandoned, while its inhabitants continued as a community, cultivating the land and living elsewhere. M. Zerner demonstrates for the Comtat Venaissin in 1414 that a very high percentage of households owned some land, if only a tiny garden, while none save the notary and local lord possessed more than five hectares.

Two articles explore the administration of city governments in sixteenth-century Spain. J. Martin's research on Gata complements

that of H. Lapeyre for Valencia. The privileges and organization of municipal powers of the urban patriciate were very similar. Lapeyre implies that what he says the medievalists prefer to call "urban patriciates," but which he refers to as *grands bourgeois*, employed similar techniques of political control through guilds and other organizations and through municipal governments. B. Bennassar describes the reactions of these same *grands bourgeois* during a plague in Santander in 1596. Most fled the city for their country estates, but those who stayed provided leadership at the risk of their own lives. The few leaders who stayed also brought popular elements of the city into the municipal government to ensure public order, which suggests a relationship between crises such as plagues and democratization in late-medieval urban history.

The papers on the municipal governments of France in the eighteenth century represent by far the most valuable and original part of this volume. The focus is continually on the social aspects of provincial and urban politics. Were the provincial administrations already moribund, the city militias defunct, and the indebtedness of towns so great that they were unable to respond to the needs and violence of the 1780s? D. Ligou stresses the local autonomy that survived in the *pays d'état*, in this instance Burgundy. Power over taxation was shared by intendants, governors, provincial estates, Parlement, and the municipal governments, giving some freedom of action to the latter. In another *pays d'état*, Languedoc, conflicts between the municipal government of Toulouse and the Parlement in the same city became intense. The intendant and the royal ministers in Paris scarcely dared to intervene. The Parlement gained in power at the expense of the municipal government and of the Crown as well, which signaled the events to come in 1789. To indicate further the weakening of the Crown over local institutions, M. Bordes points out the failure of the Crown's efforts to sell newly created municipal offices in Languedoc in 1771. Such a policy was anachronistic and ineffectual since there were few purchasers of these offices.

Nor did new institutions create an enthusiastic response. In Touraine, where municipal councils were established in 1787, M. Bouloiseau

describes the confusions over such complications as electoral procedures and the lack of a meeting place. Unwillingness to serve without remuneration hampered the effectiveness of these councils and, on the basis of the social origins of their members, suggests that for the *laboueurs* (the biggest single group) and others, time spent in politics meant a loss of income. Even so, these individuals gained some political experience as they grappled with local problems, thus preparing them for writing the *cahiers* in 1789. Y. Castan suggests that the peasantry around Toulouse continued to look to the nobility for the initiative in solving local problems.

A. Corvisier discusses a more specifically urban phenomenon, the bourgeois militias, and finds that they continued to exist in the eighteenth century. Their value as soldiers was low, but they were still guarantors of order in cities as large as Montpellier and Bordeaux. Corvisier suggests that the "bourgeois," including the *gros bourgeois*, gradually came to see bearing arms in guard duty as dishonorable. But as the need for order became apparent in 1789, national guards simply assumed the role that had previously been exercised by the militia.

J. Solé promises much under the flashy title of "Carnal Passion and Urban Society in the Ancien Régime" for Grenoble during the reign of Louis XIV, but after stating how important it is to study the history of sexuality, he describes the familiar and much more limited subject of prostitution in a very impressionistic and old-fashioned way.

The concluding articles in the book are about the definitions of the notables and bourgeois who continued to exercise so much influence on every aspect of French life in the early nineteenth century. J. Vidalenc stresses how these propertied classes moved back and forth from town to country. P. Gonnet defines the characteristics of the bourgeois more precisely as having domestic servants, rural domains, investments including houses they rented, and money invested in furniture, jewels, and gold. Whether the income is from rural or urban sources makes little difference in the style of living and the influence of the bourgeois in society. M. Agulhon, by studying the decline of small towns in the department of

the Var of the nineteenth century, argues that the middle-income but quite well-educated bourgeois families of these small towns migrated elsewhere and furnished the personnel for the administrative and industrial cadres of modern France. Finally, A. J. Tudesq examines in detail the results of the municipal law of 1831 on national French politics.

OREST RANUM

Johns Hopkins University

LAWRENCE C. WROTH. *The Voyages of Giovanni da Verrazzano, 1524-1528*. New Haven: Yale University Press for the Pierpont Morgan Library. 1970. Pp. xvi, 319, 46 plates. \$25.00.

This volume is a fitting crown to the late Dr. Wroth's many years of devoted and productive scholarship. It began with his being commissioned by the Pierpont Morgan Library to prepare a new edition of one of its treasures, the Cellère Codex of Verrazzano's contemporary letter on his North American voyage to the king of France. This is the core of the book, which includes a facsimile of the manuscript, notes on variations from other copies by Frederick B. Adams, Jr., a transcription of the Italian text, a fresh English translation by Susan Tarrow, and several pages by the author on the origin and provenance of this manuscript. Considering that the Cellère Codex has only twice been printed, and in hard-to-come-by publications, this section alone gives full value to the book.

In addition Dr. Wroth has given us a fresh biography of Verrazzano, the geographical background of and preparation for his voyage of 1524, and a narrative of the voyage, identifying so far as possible the places mentioned in the letter and on the maps by Maiollo, Girolamo da Verrazzano, Gastaldi, and others. He transcribes all known additional sources on Verrazzano's voyages (such as the Fécamp documents and the Giovio poem); he gives us a critical account of his tragic death at the hands of cannibals and has a long chapter on the influence of his North American voyage on geography. This last is illustrated by forty-six plates of earlier and later maps that, owing to the generous format of the book and the excellent printing, are big enough so that with the aid of a glass one may read almost every

name and inscription. Almost completely new is the chapter on "the Chabot Project"—the voyage of 1526-27 to Brazil, set up by Philippe Chabot de Brion, admiral of France, later the patron of Jacques Cartier. There are also notes on the portraits and a complete bibliography.

Henry C. Murphy's charges in 1875 that the voyage of 1524 was spurious and that Verrazzano was really a French pirate were long ago disposed of, but there are plenty of other controversies about Verrazzano with which Dr. Wroth has dealt carefully. First, where was he born, and to what parents? On this point Dr. Wroth leaves us uncertain whether his hero was born at the family *castello* (still standing) near Greve in Chianti or to a Florentine mercantile family established at Lyons. In either case it is certain that the explorer was an Italian of gentle breeding and classical education and that his voyage was financed, at least in part, by Lyons merchants looking for a new route to the Oriental sources of silk.

It so happens that I made an independent study of the voyage of 1524 (*European Discovery of America: The Northern Voyages* [1971]) in which I differ somewhat from Dr. Wroth's place identifications. These are few and inconsequential, except perhaps the location of Verrazzano's "Archadia," which Wroth places north of the Chesapeake Bay and I place at Kitty Hawk, North Carolina.

In every respect it was a remarkable voyage, both for what the navigator saw or thought he saw, and for what he missed; and he missed a lot because, doubtless for safety's sake, he generally sailed far off shore and avoided all harbors except New York and Newport, anchoring by preference in open roadsteads. His latitudes were the most accurate taken by any transatlantic explorer prior to Cartier. His assertion at the end of his letter that the coast was continuous from Florida to Cape Breton and that it was part of a really new world "not linked with Asia or Africa," cancels out his earlier mistake of taking Pamlico Sound, North Carolina, for an arm of the Pacific Ocean.

Is there, then, anything more to be done on Verrazzano? Yes indeed! Someone should sail in the spring of the year over his entire course from South Carolina to New York, letter and earliest charts in hand, to identify the many

names that have puzzled every historian. And nobody has yet surely identified the Indian tribes of the Carolina coast whose manners Verrazzano so vividly described. Until these things are done, Dr. Wroth's really great book must be considered definitive and his scholarly conclusions unchallengeable.

S. E. MORISON

Harvard University

LESTER S. KING. *The Road to Medical Enlightenment, 1650-1695*. (History of Science Library.) New York: American Elsevier. 1970. Pp. x, 209. \$11.50.

It is difficult to characterize a well-defined era such as the Renaissance or the French Revolution; Dr. King here attempts an even more complex task—that of analyzing an interim period, a kind of medical *prérévolution*, extending from the death of Galileo to the birth of Voltaire, from Cromwell to Queen Anne. It is an era flanked by giants in medicine, Harvey and Boerhaave; and in the philosophy and methodology of science, Bacon and Locke. Sir Thomas Browne was the typical enlightened physician of this age; Tyson, Swammerdam, and Malpighi its exceptional experimenters, and Molière's doctor the parody of its practitioner.

King is not concerned with the giants. Rather he wishes to analyze medical thought in the context of the "general cultural milieu" (p. 7). To do this he chooses the metaphor of the "Road to Enlightenment," leading past a number of way stations. At each we are presented with the scholarly portrait of a typical doctor: Riverius, a strict Galenist and a successful practitioner at Montpellier in the days of Henry IV and Richelieu; Van Helmont, the famous neoplatonic alchemist and inventor of the term "gas"; Boyle, the Baconian and "atomist," an early devotee of pharmacology.

After analyzing the metaphysical positions, King explores methodological attitudes in the persons of Sylvius and Sydenham—one an iatrochemist, the other an empiricist and classifier. The "shifting patterns" described in chapter 4 lead to the culminating point: Friederich Hoffmann's *Fundamenta Medicinae* of 1695, which Dr. King has translated into

English for the first time. And although Hoffmann "did not provide any new discoveries," he is King's choice to illustrate and typify half a century of medical thought. The *Fundamenta* "shows the persistence of old ideas under the guise of the new," "exemplifies system-formation," and "discloses the conflict between different ideas and lets us see the way that the struggle was temporarily resolved" (pp. 182-83).

Bypassing the great doctors and scientists, King is able to focus on an intelligent medical practitioner who glimpsed, but did not quite see, the "light" of the Enlightenment. This book is thus an analysis of the intellectual and medicoscientific background of a typical late seventeenth-century physician, uninterested in microscopic research, unaware of such new fields as comparative anatomy and embryology, evidently untouched by Newton and Locke. King proposes the *Fundamenta* as "the best single expression of the new medicine," adding that it "exemplifies the complex currents" (p. 181). He provides us with an additional facet to the struggle of the "Ancients and Moderns," a prelude to his *Medical World of the 18th Century* (1958), a vivid reminder that the history of medical thought forms a lively part of the Western intellectual tradition.

DORA B. WEINER

Manhattanville College

HENRY BLUMENTHAL. *France and the United States: Their Diplomatic Relations, 1789-1914*. Chapel Hill: University of North Carolina Press. 1970. Pp. xiv, 312. \$9.75.

LYNN M. CASE and WARREN F. SPENCER. *The United States and France: Civil War Diplomacy*. Philadelphia: University of Pennsylvania Press. 1970. Pp. xv, 747. \$22.50.

Throughout the nineteenth century the major foreign policies of France and the United States tended to separate the two nations. Following the War for American Independence, France was primarily concerned with restoring its prestige and power on the Continent, and the United States devoted itself to the construction of a continental empire in political isolation from Europe. At the same time, however, both nations had important commercial ties and were concerned with the world balance of power. As a consequence, France could never

ignore the United States, and the United States could never achieve real political isolation. In addition Americans were committed to the romantic notion of a special relation between France and the United States stemming from French support for the rebellious American colonies. The functions of commerce, geopolitics, and emotional attachment gave a peculiar flavor to nineteenth-century Franco-American relations and are ably explored in these two studies.

Henry Blumenthal's major contribution lies in providing needed perspective. His chief argument is that whatever affinity Frenchmen and Americans may have felt, the diplomatic relations between their nations were essentially determined by coldly realistic commercial and geopolitical calculations, both of which were closely intertwined in French thought. French policy, shaped by the fear that America would upset the European balance of power to France's detriment, vacillated between cultivating and antagonizing the Americans. Similarly, the United States attempted to use the European balance to further its own ends. While at times France and the United States could work to mutual advantage, more often their relations were severely strained. The myth of the special relation served chiefly as an anodyne to these frequent tensions.

Blumenthal's study falls well within the "realist" school, and while a few of his specific interpretations are questionable, for the most part he is persuasive. He has provided an excellent synthesis that incorporates insights from the most recent and well-known studies with much new material from archival and contemporary sources.

Lynn M. Case and Warren F. Spencer have concentrated their interest on the Civil War years. Their research has been exhaustive, and this work, comparable to E. D. Adams' study of Anglo-American Civil War relations, is clearly definitive. Case has covered the early years of the war to the resignation of foreign minister Thouvenel in late 1862; Spencer was responsible for the remaining years and the ministry of Drouyn de Lhuys.

Case's and Spencer's general interpretation is similar to Blumenthal's. Added is considerable depth and detail and a number of important new interpretations of specific episodes.

Analyses of Seward's diplomacy, the effect of the Civil War on the Anglo-French entente cordiale, the French contribution to the peaceful settlement of the Trent affair, the effectiveness of the Union blockade, and the importance of the disruption of Franco-American trade on French policy are fresh, convincing, and well done.

This study is not without some weaknesses. While the analyses of the diplomacy of Napoleon's foreign ministers are excellent, the enigma of Napoleon III remains largely unsolved. Blumenthal's broader treatment is superior. Also, Case's contention that the Trent affair may have been a Confederate plot to bring about an Anglo-American war is intriguing but not wholly convincing. And finally, the stylistic device of converting material from diplomatic correspondence into direct discourse does not substantially enrich the literary value of the work (its only possible justification), while it creates some distortion and reduces the book's usefulness.

On the whole, these two excellent works are major contributions to the literature of Franco-Americans. Both provide valuable insights, perspective, and new material necessary for understanding the unique relationship between France and the United States, both past and present.

KINLEY J. BRAUER
University of Minnesota

GEORGE E. BROOKS, JR. *Yankee Traders, Old Coasters, & African Middlemen: A History of American Legitimate Trade with West Africa in the Nineteenth Century*. (African Research Studies, Number 11.) [Brookline, Mass.:] Boston University Press. 1970. Pp. xiv, 370. \$12.50.

George Brooks's monograph, designed for the specialist in either nineteenth-century American or African economic history, is a valuable addition to the growing literature dealing with United States-African relations in the last century. With scholarly detachment and consummate concern for detail, Brooks traces American "legitimate" trade with West Africa through three major stages of development: its late eighteenth-century origins, as New England merchants replaced English and French sources cut off from Africa by the Napoleonic Wars; the era of mercantilist ex-

clusion (1815-30), when Americans resorted to smuggling in order to continue their commerce with European colonies on both the Leeward and Windward coasts; and the golden era of free trade (1850-80), when large numbers of American vessels freely traded along Africa's west coast.

At best, American "legitimate" trade with Africa was limited. Such trade as did exist was confined largely to the exchange of American rum, tobacco, guns, and ammunition—essentially luxury items—for hides, palm oil, ivory, cam wood (for making dyes), and, later on, peanuts. Brooks concludes that while Africa consumed a minuscule percentage of American exports, one per cent at its peak during the 1860s, American goods represented a considerable portion of foreign products flowing into West Africa in the nineteenth century.

While no one can quarrel seriously with Professor Brooks's conclusions, his designation "legitimate trade" in the subtitle of his study is open to question. Professor Brooks defines "legitimate trade" as trade encompassing any commodity except slaves. Yet until 1807 the slave trade was a "legitimate" enterprise for English merchants, and their American counterparts could "legitimately" engage in similar activities until 1809. Moreover, Professor Brooks catalogs the extensive smuggling activities of the Yankee merchants engaged in the West African trade in the post-Napoleonic era. They violated the English Navigation Acts and the French *Exclusif* with impunity in their pursuit of "legitimate" trade. To consider a trading venture that must rely for its success on constant circumvention of the laws as legitimate is a peculiar treatment of legitimacy.

Finally, this book does not touch upon American trade with Liberia, the African nation of greatest interest to Americans in the nineteenth century. Professor Brooks tells us that the subject is too vast and complicated to be presented adequately in the present study, and that he will deal with Liberia in a separate volume. We hope this essential companion study will equal the high scholarly aspirations of Brooks's current work.

SHELDON H. HARRIS

San Fernando Valley State College

G. L. BONDAREVSKII. *Angliiskaia politika i mezhdunarodnye otnosheniia v basseine Persidskogo Zaliva (konets XIX-nachalo XX v.)* [England's Policies and International Relations in the Persian Gulf Basin at the End of the 19th and the Beginning of the 20th Century]. (Akademiia Nauk SSSR, Institut Narodov Azii.) Moscow: Izdatel'stvo "Nauka." 1968. Pp. 541.

For well over a century Great Britain was firmly in control of the Persian Gulf. The British navy patrolled its steamy waters, and the British resident at Bushehr exercised a quasi-monarchic authority over its shores. British hegemony was not unchallenged, however. The French made some feeble attempts to establish themselves at Muscat. The Germans, whose influence at Constantinople increased rapidly in the decades immediately preceding World War I, tried to penetrate Kuwait. Russia, having acquired predominance at Tehran through control of the shah's purse, looked forward to the eventual annexation of Persia and showed her flag in the Gulf.

G. L. Bondarevskii, author of an earlier study touching upon the problem of international rivalry in the Persian Gulf (*Bagdad-skaia doroga i proniknovenie germanskogo imperializma na Blizhnii Vostok* [1955]), analyzes British policies toward the coastal states and toward the great powers in the late nineteenth and early twentieth centuries in the spirit of old Anglo-Russian rivalry. The concluding paragraphs of the book purport to show the activation of British imperialism in the Gulf in the late 1960s, which had as one of its manifestations "the publication of scholarly research and popularizations whose authors strive to prove that Great Britain has made a great contribution to the economic development of the Persian Gulf, that it was she who saved the people of those countries from the horrors of slavery and the slave trade and from enslavement at the turn of the century by Turkey, Iran, Germany, and especially Tsarist Russia." Thus inadvertently the author reveals the essential purpose of his own work: to show that Great Britain has enslaved the nations of the Persian Gulf and that her policies have not changed to this day, even if she is about to withdraw from the area the last remnants of her once great naval forces.

Stripped of its Marxian terminology, this

book would have been worthy of any late nineteenth-century Russian patriotic writer. K. Skalkovskii or I. A. Zinoviev would not have hesitated to endorse it. Though largely polemical, the book provides a detailed review of many obscure events culled from the relatively large literature in Western languages. The total absence of references to sources in Arabic, Turkish, or Persian is striking. The author relies heavily on published collections of documents and the research of such scholars as R. Kumar, J. B. Kelly, and J. B. Plass. He has not explored British archives, all of which are open to scholars, and his use of materials from the Russian archives, which are closed to outsiders, does not add much to one's understanding of British policy. The system of transliteration, the bane of all who write on the Middle East, is inconsistent and confusing. The style is pedestrian, and neither events nor persons ever come to life.

FIRUZ KAZEMZADEH
Yale University

M. F. IUR'EV. *Revoliutsiia 1925-1927 gg. v Kitae* [The Revolution of 1925-1927 in China]. (Moskovskii Gosudarstvennyi Universitet im. M. V. Lomonosova, Institut Vostochnykh Iazykov.) Moscow: Izdatel'stvo "Nauka." 1968. Pp. 518.

This book, written in Russian, is published under the sponsorship of the Institute of Eastern Languages, Moscow State University of Lomonosov. By reconstructing the events of the "Great Chinese Revolution" of the 1920s, Iur'ev shows why after initial successes the Soviet effort to turn China into a Communist province ended in failure.

The book consists of four parts. Iur'ev first discusses the foundations of the revolutionary forces in Canton and the intensification of the general revolutionary situation in China from January 1924 to May 1925. He then deals with the escalation of the revolution in major cities of China from May 1925 to July 1926, the incident of May 30 in Shanghai, and the end of the united front. The third part of the book discusses the Northern Expedition (with special reference to its first and second phases), which resulted in military conflicts between the "proletariat" and the "imperialists and Chinese

reactionaries" in Shanghai. In the fourth part the author particularly emphasizes the defeat of the revolution in Central China after the "treason" of Chiang Kai-shek.

The author realizes the failure of Stalin's effort to convert the Nationalist Revolution into a social revolution patterned on the Soviet model. He also stresses, however, that this failure actually led to the final success of the domination of the mainland by the Chinese Communists. In the concluding passage he states, "The defeat of 1927 led to a temporary recess of the political activities of the labor and peasant classes. Gradually, however, a new form of struggle began to move forward. The fine traditions and the new experiences of 1925-1927 continued to inspire the Chinese people, helped them to lead a difficult and long struggle with imperialism and inner reactionaries. . . . The participants in the revolution of 1925-1927 made a tremendous contribution to the final triumph of the democratic revolution in China that occurred in 1949" (p. 517). The author gives the account entirely from the Soviet point of view, which is not well balanced. The book would be more revealing if it were read along with another work of similar nature, such as *Stalin's Failure in China, 1924-1927* (1958) by Conrad Brandt, a brief but most rewarding analysis of the abortive Soviet effort to bring about a Communist revolution in China in the 1920s—a study that has placed events in a more proper perspective.

PAUL K. T. SIH
St. John's University

JOSEPH I. LIEBERMAN. *The Scorpion and the Tarantula: The Struggle to Control Atomic Weapons, 1945-1949*. Boston: Houghton Mifflin Company. 1970. Pp. xiv, 460. \$8.95.

Now, more than a quarter century after Alamogordo, the subject of international control of atomic energy, 1943-49, is ripe for sustained historical analysis, particularly with the publication of the first two volumes of the history of the Atomic Energy Commission and the efforts by scholars to pry back the lid on federal documents of this period. There is a need to reassess the Baruch plan, to examine why American leaders would not admit publicly

(and seemed sometimes not to understand) that the plan protected the American nuclear monopoly, and to explain why Soviet spokesmen were so slow in raising this fundamental objection and why they dwelled at first on the problem of the veto. The probing historian must also explain why there was so little cooperation between the State Department and the military on atomic energy and how military strategists and policy makers in the years after Potsdam thought the bomb could be used to advance the interests of the United States and to deter Soviet expansion. Put simply, what was the nature of "atomic diplomacy," and was NATO a response partly to the belated recognition that American nuclear weapons would not be effective in rolling back Soviet troops, if they expanded, from Western Europe? Furthermore, when did Soviet leaders, perhaps accepting the analysis of Eugene Tarle, recognize the limitations of the nuclear monopoly outlined by Walter Lippmann in 1946 and P. M. S. Blackett in 1948?

In this heavily derivative, poorly researched, erratically documented study, Joseph Lieberman, a Yale Law School graduate who previously published his undergraduate thesis, has failed to explore in depth most of these important questions. Much of his volume is little more than a popularized summary of the fine official history of the AEC by Richard Hewlett and Oscar Anderson. Lieberman has supplemented their work with occasional references to the *New York Times*, a few Soviet newspapers, Margaret Gowing's study of atomic energy in Britain, Alice K. Smith's volume on American nuclear scientists, and some of the memoirs of policy makers and government advisers. Doing less research in the available sources than Hewlett and Anderson, Lieberman arbitrarily and unwisely limited himself to the papers of Henry L. Stimson, William Leahy, Vannevar Bush, J. Robert Oppenheimer, David Lilienthal, Bernard Baruch, and some unclassified documents of the AEC. He neglected the collections at the Roosevelt and Truman libraries, as well as the papers of James Forrestal, Robert Patterson, and John Foster Dulles, the *Public Papers* of Truman, and the memoirs of Dean Acheson. Even Lieberman's bibliography is defective, for he

does not cite Gar Alperovitz' *Atomic Diplomacy* (1965), on which he seems occasionally to rely; Gabriel Kolko's *Politics of War* (1968), which suggests important insights about the Anglo-American nuclear relationship; Adam Ulam's *Expansion and Coexistence* (1968), which contends, contrary to Lieberman, that Soviet leaders were not deeply fearful of the American nuclear monopoly; and the second volume of the AEC history, which devotes more than three hundred pages to the three-year period (1947-49) that Lieberman superficially sketches in eleven pages.

Aside from some trivial mistakes, this book also suffers from a severely limited focus and restricted analysis. Lieberman only loosely considers the military strategy in 1946-48 that compelled American policy makers to rely upon the bomb when the huge wartime army melted away under the demands for demobilization. Only in the final chapter does he provide much analysis, and even there his comments are often very brief. He finds that American objections to a Soviet sphere in Eastern Europe seemed incompatible with the United States sphere in Latin America; that American leaders were foolish not to inform Russia of the bomb and to invite her into a nuclear partnership before the end of the war, particularly since they knew that the Communists had learned of the Manhattan Project and could soon develop a nuclear capacity; that the Acheson-Lilienthal plan erred by stressing American security and by minimizing concern about Soviet acceptance; and that the Baruch and Acheson-Lilienthal plans were unacceptable to the Soviets primarily because these plans protected the American nuclear monopoly for some years and required an international presence in the Soviet Union. Lieberman's conclusion, which is hardly original, is that the failure to agree on international control of atomic energy was caused by, and a cause of, "broad and selfish misunderstanding and conflict between the Soviet Union and the United States." Taking his title from Louis Halle's likening of the cold war to "the scorpion and the tarantula" placed together in a bottle and trying to kill one another, Lieberman agrees with Halle that the cold war "is not a case of the wicked against the virtuous." In places, however, Lieberman seems

to disagree with Halle's assessment that "both parties . . . [were] in a situation of irreducible dilemma," for Lieberman in his last chapter generally emphasizes what the United States, not the Soviet Union, should have done to avoid discord on atomic energy. Unfortunately, Lieberman's last chapter, tantalizing and thoughtful, is scanty compensation for this otherwise disappointing book.

BARTON J. BERNSTEIN
Stanford University

B. N. PONAMEREV, editor. *Mezhdunarodnoe revoliutsionnoe dvizhenie rabocheho klassa* [The International Revolutionary Movement of the Working Class]. Moscow: Izdatel'stvo Politicheskoi Literatury. 1966. Pp. 446.

Boris Ponamerev is really one of the more competent historians of modern times in the Soviet Union. Yet part of his prominence lies in his occasional editing of official party histories or political works, and such works serve to vitiate his credentials as a serious historian. The present lightweight collection of essays on the international proletariat movement is unfortunately such a work. The book is a compilation of case studies by several Soviet "party and scholarly" personnel—all of whom have the good fortune not to have their names associated with their specific contributions.

The scholarship can best be described as Komsomol standard—perhaps the subject matter does not allow any other yardstick. The documentation consists largely of the same wearisome quotes from Marx and Lenin, despite the contemporary nature of the subject material. Substantial research is apparent neither in the documentation nor in the findings. There are occasional perceptions such as the reluctant admission that the antiwar movement in the United States is not a proletarian movement—at least not in 1966—a phenomenon rationalized by references to venal union leaders.

The book is perhaps more noticeable for what it omits, any serious consideration of the role and impact of Red China on the international movement, for example. What is more critical, however, is the timid handling of the relationship between the Soviet Union and the international proletarian movement. Here all of the authors affect the greatest

delicacy and essentially revert back to the Stalinist line of the 1930s: the greatest service that the Soviet Union can render the international proletariat movement is to achieve such economic strength that it will be an inspiration to workers everywhere. No promises of direct aid to the international proletariat are offered; if anything, advice is on the side of caution. All proletarians are assumed to have a common struggle against thermonuclear war, colonialism, and similar evils. All proletarians are expected to enjoy vicarious triumph in the achievements of the Soviet Union. The message of the book seems to freeze proletarian solidarity at that level.

WARREN LERNER
Duke University

ANCIENT

KURT BITTEL. *Hattusha: The Capital of the Hittites*. New York: Oxford University Press. 1970. Pp. 174, 30 plates. \$10.00.

Judging from the ongoing printed accretions in many languages, "The Secret of the Hittites" remains a highly marketable commodity. Most such works, epitomized by the anagrammatic Marek-Ceram's book of that name (1956), have been popularizations, however readable and serviceable, from the pens of second-hand sifters or journalistic travelers. It is therefore a welcome event to get at length a comprehensive, direct statement from the distinguished excavator of the ruins of Bogazköy since 1931, based on the Mary Flexner Lectures given by Dr. Bittel at Bryn Mawr College in 1967.

Bittel's account carries the conviction and immediacy of one who has devoted the better part of a long and distinguished career to that magnificent archeological monument. He knows the site like no one else, and he is able to extract impressive quantities of archeological information and place it in a diatopic and diachronic Anatolian context. More important still, he unerringly collates the philologically extracted information from the royal archives (many of whose tablets he has himself excavated) with the observable data of the site and the ruins and thus achieves a task of comprehensive historical reconstruction.

The first chapter probes the site itself in terms of locale, historical identification, early

exploration, and settlement history. The second surveys the city as the visible remnant of the capital of a long-forgotten kingdom in its development during the better part of the second millennium B.C. The third concentrates on the citadel area of Büyükkale as the royal nucleus of the empire in the fourteenth and thirteenth centuries. The fourth explores the nearby open-air rock sanctuary of Yazilikaya and stresses the funerary, even sepulchral aspects of its cultic identification. After a discussion in the fifth chapter of Hittite-Egyptian relations, which somewhat diverts the focus of the volume, Bittel concludes with an illuminating survey of the history of the Bogazköy site in the post-Hittite, Phrygian, and Persian eras of the first millennium B.C. The size of the city in the seventh and sixth centuries B.C. makes possible after all an identification with Herodotus' Pteria, destroyed by Croesus in 547; thus are vindicated the hunches of early nineteenth-century A.D. identifiers, which were subsequently submerged in the triumphant recognition of the second-millennium Hittite capital.

Bibliography, index, and plates bring up the rear. The curious reader is guided to a mixture of general and specialized further sources. Bittel began chapter 1 by copious reference to the most important new Hittite text found in more recent years, the autobiography of King Hattushili I (ca. 1600 B.C.). He somehow withholds the information that this historically important Hittite-Akkadian bilingual is now available in the edition and translation of Fiorella Imparati and Claudio Saporetti in volume 14 of *Studi classici e orientali* (1965).

JAAN PUHVEL
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D. R. DICKS. *Early Greek Astronomy to Aristotle*. (Aspects of Greek and Roman Life.) Ithaca: Cornell University Press, 1970. Pp. 272. \$7.50.

The present volume, part one of a two-part history of Greek astronomy planned by the author, covers the period from Homer to Aristotle and thus brings the reader to the threshold of the Hellenistic Age and the dawn of mathematical astronomy. Excluding the introduction

and some fifty pages of copious but useful notes, about one-third of this book is devoted to the Pre-Socratics and Pythagoreans, another third to Plato, and a final third to Eudoxus, Callippus, and Aristotle.

The Cornell series on "aspects of Greek and Roman life," which professes to serve scholars, students, and the general reader, has presented several of its authors with a Procrustean bed into which certain subjects simply will not fit. In the case at hand the scholarly audience potentially would consist of classicists, those interested in ancient philosophy, and the professionals in the history of science. This is a group so diverse that an approach satisfactory to one segment would not please another. Professor Dicks, therefore, has chosen wisely to address himself to the classicists and philosophers, the people most in need of the kind of instruction he has to offer. As a consequence he has also provided a book that will be useful to graduate students and of interest to general readers who have already cultivated an interest in the subject.

There are a number of good things to be said about this volume. It is the first book in English devoted exclusively to the subject since T. L. Heath's *Greek Astronomy* forty years ago; needless to say, the world has moved on since then. In addition, anyone who has tried to separate fact from fiction in the ancient accounts of the Pre-Socratics will appreciate the sensible remarks of Professor Dicks on this matter. Equally balanced and sensible is his assessment of the relationship of Greek to Babylonian astronomy in the light of our present knowledge, though admittedly the picture may change at any moment. Particularly welcome is the chapter on Eudoxus, a major and somewhat neglected figure in the story of early Greek science.

While the author has not convinced me of the importance of Plato to Greek astronomy, this chapter will be eagerly read, given the audience he has chosen for himself. Further, considering the differing frames of reference of "Homer" and Hesiod, it is doubtful that Hesiod's "correlation of astronomical phenomena with seasonal variations" is truly evidence for an advance in Greek thought from the time of the one author to that of the other (p. 34). And one last minor point: it seems to

me that the Mesopotamian omen texts suggest that "judicial astrology" is older than circa 1000 B.C. (p. 28).

TOM B. JONES
University of Minnesota

JACK LINDSAY. *The Origins of Alchemy in Graeco-Roman Egypt*. New York: Barnes and Noble. 1970. Pp. xi, 452. \$10.00.

Mr. Lindsay gives first an outline of the Greek scientific ideas through which a theory of metallic transmutation could arise. Then, after two interesting chapters on historical references to alchemy—some rather farfetched—he deals with the major Greek alchemists as we know them from Berthelot, providing a convenient summary of the scholarship on Bolos of Mendes, "Democritus," Ostanos, Maria, and others. Not all the scholarship is surveyed, however; Jung's views do not appear, and Mr. Lindsay dismisses the Chinese claim to priority altogether too brusquely. In other cases, such as his accounts of Roman laws on counterfeiting, and of secrets discovered in tombs or pillars or inscribed in unknown languages on columns, the scholarship is overdone and tedious. Indeed, an apparent unwillingness to leave anything out is the main fault of a book designed with the general reader in mind, and the result is often clumsy management of material—as in chapter 11, "Mary the Jewess," which begins with over three pages on oddments not obviously related to her.

Paradoxically, however, such side material, though often excessive, turns out to be one of the book's most useful features. Mr. Lindsay can hardly be expected to have found fresh material, or to provide more exact knowledge of chemical actions in the furnace or *kero-takis*; here he is necessarily derivative. But the details of ancient Near Eastern thought systems, religions, magic, charms, prayers, rituals, crafts, and daily living feelingly persuade us of the mental milieu of the era when alchemy appeared. This appearance, the author argues, is due to a coming together of Greek, Egyptian, and Persian ideas with the metalwork and imitative jewelry crafts. He presents the usual picture of the alchemists reducing their material to a primary, often fluid, state, operating on it with other substances having "sym-

pathy" with it, moving it to higher stages of perfection characterized by the color sequence of black, white, yellow, purple (corresponding to a color symbolism in certain mystery religion resurrection myths), ultimately to reach the perfection of gold. All the imparted qualities causing these colors are due to a tingeing pneuma. He sees the early alchemists' unique distinction as alone among their contemporaries holding to a fully materialist view of the universal pneuma, not allowing the "superiors" to become spiritual and radically separate from matter. This is hard to accept; spirit as a kind of tenuous matter had a long history—witness Milton's angels and God. As a final note the book would have greatly benefited from a less opaque set of notes and bibliography.

JOHN REIDY
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H. H. SCULLARD. *Scipio Africanus: Soldier and Politician*. (Aspects of Greek and Roman Life.) Ithaca: Cornell University Press. 1970. Pp. 299. \$6.95.

This book makes available in a single attractive volume the results of Professor Scullard's considerable research on the life and times of Scipio. His two earlier books on this subject are now out of print: *Scipio Africanus in the Second Punic War* (1930) and *Roman Politics, 220–150 B.C.* (1951). The present book appears in the Aspects of Greek and Roman Life series of which Professor Scullard is the general editor, a series written for scholars, students, and general readers. It shows some modifications of the author's earlier views and incorporates recent scholarship in the field. The text is lively and moves along at a good pace. The notes at the end of the book are generous; some are small appendixes. They are for the most part devoted to new material or interpretations; the author's two earlier books are cited frequently for problems previously discussed. Considerable attention is given to the topography of battle sites. Numismatic evidence is also carefully considered. The book is the richer because Professor Scullard was able to utilize the advance proofs of Professor Walbank's *Historical Commentary on Polybius*, volume 2. The plans, maps, and photo-

graphs of coins and battle sites add to the usefulness of the text.

Scipio Africanus is one of the most significant figures in Roman history. At a time of great military need during the Hannibalic War, the Roman people took an unprecedented step and for the first time granted proconsular *imperium* to a young man who had not yet held *imperium*. Furthermore his command was continued for the duration of the war. The command of young Scipio marks an important stage in Rome's constitutional development and was to have far-reaching consequences. Events, however, soon proved the wisdom of selecting the best person irrespective of his technical qualifications. Through his tactical reforms of the Roman army, Scipio forged a weapon capable of eventually defeating Hannibal on his own territory. The tactics developed at Baecula, Ilipa, and the Great Plains prepared the way for Zama. Scipio's sympathy for the native point of view, the revolutionary importance of the settlement of his veterans at Italica, his magnetic personality, his later role in Eastern affairs, and the importance of his philhellenic sympathies are all discussed.

The author has a great admiration for Scipio, but he does not hesitate to recognize that there are some blots on his character. Unlike the rationalist Polybius, who overreacted against the popular view, he correctly interprets the personality of Scipio against the background of the religious atmosphere in which he lived. Throughout the book the career of Scipio is always discussed in the context of the complex background of the history and culture of the period. "A king without a kingdom, he gained for Rome supremacy throughout the length of the Mediterranean; in Spain, Africa, and the East. He championed Rome's imperial and protectorate mission in the world. Vested with proconsular power, which was the mainstay of the later emperors, he was great enough to inspire some with a belief in his divine inspiration—a belief accorded to many an emperor only by servile flattery."

Professor Scullard has given us a book that will serve scholar and undergraduate alike for some time to come.

WILHELMINA F. JASHEMSKI
University of Maryland

PETER GARNSEY. *Social Status and Legal Privilege in the Roman Empire*. New York: Oxford University Press. 1970. Pp. xiii, 320. \$10.50.

This interesting book deals with inequalities in the Roman legal system from the time of Cicero to the Severan emperors. The thesis is that men of status, the possessors of *dignitas* (status derived from political power, wealth, and style of life), enjoyed significant legal privileges denied to others in the Empire. Garnsey is aware that his thesis is not original. The value of the book is in the cogent analysis of the development and use of legal privileges.

The author concentrates on both *de jure* and *de facto* inequality. *De facto* inequality in Roman law—the ability of the wealthy and the influential to make better use of the legal system—receives less treatment because it is well known. Greater emphasis is placed on *de jure* inequality, legal discrimination in favor of men of status. The discussion of the dual-penalty system—the establishment of milder punishments for men of *dignitas* than for others—is especially good.

In the last part of the book the author studies the privileged groups of the Roman Empire (senators, equestrians, decurions, and veterans) and attempts to explain their privileges and the basis for them. There is an interesting chapter on the privileged groups as a whole (the *honestiores*) and the rest of the Roman citizens (the *humiliores*) showing how citizenship per se became less valuable in the period from Augustus to Caracalla while high status became more valuable. In general this part of the book is not as fully developed as the earlier parts.

The book contains a few statements of dubious value, and there are some surprising omissions. Tiberius' attitude toward the treason law is described as "liberal," in the sense that he wanted to keep it "within bounds," but this is hard to reconcile with some of the other arguments the author makes about Tiberius. The exile of Seneca and Julia Livilla on the charge of adultery is not mentioned in the sections on adultery or exile. There is no discussion of Claudius' notorious activity in the courts. Finally, it is doubtful that "the man of small means and little *dignitas* might well find beating preferable" to a monetary fine.

But these are quibbling criticisms of a good book. It is well documented, especially with primary sources, and in general it is a valuable contribution to the field.

ARTHER FERRILL

University of Washington

RADU VULPE and ION BARNEA. *Romanii la Dunărea de Jos* [The Romans on the Lower Danube]. (Bibliotheca Historica Romaniae, Number 4. Din Istoria Dobrogei, Volume 2.) Bucharest: Editura Academiei Republicii Socialiste România. 1968. Pp. 590. Lei 42.

This book describes in well-documented detail the Roman advance to, assimilation of, and presence in the area of the Lower Danube. It deals particularly with Dobruja, the region between the Danube and the Black Sea, but pays much attention also to the mountainous Carpathian region across the river, repeatedly stressing the geographic and ethnic unity of the two regions (see, for example, p. 23). The first and larger part of the volume (pp. 11–365) is by Professor Vulpe and deals with the period before Diocletian. In the absence of continuous ancient accounts Vulpe weaves together his annals of the Romanian lands by scrutinizing the information scattered in Greek and Latin authors and by adducing the archeological evidence, especially inscriptions. It is an impressive performance. Dobruja, successively part of the Odrysian client kingdom and of Roman Moesia, was a linguistic borderland where Latin, unknown there is Ovid's day (p. 40), came to prevail. By Antonine times *municipia* had been established, and the region was heavily Romanized, Greek being confined to the Pontic cities on the coast (pp. 140, 167). Meanwhile Trajan had conquered Transylvania and converted it into the Roman province of Dacia. According to Vulpe the decisive defeat of Decebalus and his allies (Buri, Sarmatae, and others) in 102 in Trajan's First Dacian War occurred in Dobruja, the three famous monuments at Adamclisi marking the battlefield—a view that is not new but restated here with conviction and authority (pp. 81–91). On the other hand, the three much-discussed walls that traverse Dobruja between Axiopolis on the Danube and Tomis on the Black Sea have nothing to do with Trajan (or the Antonines, for that matter), since all

three are hundreds of years later (pp. 75, 133, 387). The third century brought disaster (pp. 217, 248, 269), and it is in his account of the military anarchy that Vulpe's enviable command of the source materials is particularly in evidence. His descriptions of barbarian raids and folk migrations, by sea as well as by land (pp. 235–65), and his concise appraisals of "Barrack Emperors" (Maximin, p. 223; Gallienus, p. 261) are strong and vivid. Order returned with Diocletian, at which point Professor Barnea takes up the tale. With competence similar to Vulpe's he carries the story down to the seventh century, when the Romans, but not their language, had left the Lower Danube (pp. 367–556). The 160 photographs, the five careful maps, and the comprehensive index contribute greatly to the book's usefulness.

EDWARD T. SALMON

McMaster University

R. A. MARKUS. *Saeculum: History and Society in the Theology of St Augustine*. New York: Cambridge University Press. 1970. Pp. ix, 252. \$12.50.

Mr. R. A. Markus, senior lecturer in medieval history in the University of Liverpool, generously acknowledges his indebtedness to Peter Brown's magnificent study of Augustine's life and thought and to the works of other recent commentators on Augustine's ideas about the human condition, society, and politics. He endeavors, however, to distinguish his own study from others by stating that he wants to show the relation between Augustine's changing thoughts about man's ultimate destiny and developments in his reflections about history and society. In addition, throughout the volume and particularly in the final chapter, Markus tries to indicate the pertinence of Augustine's ideas to contemporary theological discussions that interest him.

Markus maintains that in Augustine's mature thought "sacred" history is unambiguously identified with the divinely inspired narratives found in the Scriptures and is sharply contrasted with "secular" history, the narratives of non-canonical authors. He notes that during the years from about 395 to 410 Augustine had jubilantly endorsed the view of

Christian writers like Eusebius and Ambrose that "the triumphant spread of Christianity, assisted by the kings of the earth, is part of God's work for the salvation of his people and the fulfilment of the ancient prophecies" (p. 32). In the years following the Gothic sack of Rome in 410, however, Augustine rejected this idea of a Christian Empire and moved steadily toward the position that no part of the history of mankind from the foundation of the Church to the end of the world can be included in the category of "sacred" history. Events in this period fall, of course, under the sway of God's Providence, but they necessarily reflect the ambiguities and the indeterminateness characteristic of human life in this world, in which the two cities, the earthly city and the city of God, while eschatologically separate, are inextricably intermingled. In this new perspective the Roman Empire becomes "theologically neutral"—"no more than a historical, empirical society with a chequered career" (p. 55).

This development facilitated Augustine's radical break with Greek and Roman conceptions of the state and of its functions. He argued that only the city of God satisfied the Ciceronian definition of the *res publica*, of which true justice is an essential constituent. Then, in order to provide a definition of the state that would be applicable to earthly states, he offered his own "positivistic" definition of a people as "an assemblage of reasonable beings bound together by a common agreement as to the objects of their love" (*City of God*, 19. 24).

While I agree with much of Markus' analysis of these important shifts and of their consequences, I am not convinced that the evidence fully supports his conclusions that Augustine's "secularisation of the realm of politics implies a pluralistic, religiously neutral civil community" (p. 173) and that "'political Augustinianism' is, of its nature, politically radical. It is bound to be unremittingly critical of all and any human arrangements, any actual or even imaginable forms of social order" (p. 168). Certainly Augustine argues that members of the earthly and heavenly cities, whose ultimate values are absolutely opposed, can agree on the importance of earthly peace and the state's role in maintaining it. But does this

position imply a "pluralistic" society, that is, one made up of a variety of groups espousing different values and beliefs? Is "political radicalism" an apt characterization of a conception of politics that forbids the Christian to identify himself or God's will with any actual or proposed social or political order and that stresses the inadequacies and injustices inevitably present in every human institution?

I am not persuaded that Markus has disposed of the difficult problem of resolving the conflict between Augustine's general conception of the state and his approval of the use of coercion by state authorities against heretics and schismatics. Finally, while I recognize that it is not easy to state Augustine's complex ecclesiology in simple terms, it is puzzling to be told, first, that the "very substance" of the Church "lies in its continuity with God's eschatological community, into which it is always growing" (p. 119), and, then, that the Church is not God's kingdom, "even in its germ or chrysalis. For there is no continuous development, no growth or maturation of the Church into the Kingdom" (p. 181).

HERBERT A. DEANE

Columbia University

MEDIEVAL

JOSEPH R. STRAYER. *On the Medieval Origins of the Modern State*. Princeton: Princeton University Press. 1970. Pp. vi, 114. \$5.00.

The history of seven hundred years of the European state, told in just over one hundred pages: on its face it looks like one more supplementary reading for "Western Civ." But it is nothing of the kind. Putting together what he has learned during thirty-five years of research and teaching, seasoned by long experience as a government adviser, Professor Strayer succeeds in summarizing a generation's work and at the same time in composing a highly personal essay.

The story as he tells it—and it is almost exclusively the story of the French and English monarchies—is by now classic in many of its formulations. How did the European states first come into being? Certain communities settled in given areas and stayed there. This allowed the time and continued contact for

them to develop specialized institutions that possessed a continuity of their own and specialized bureaucrats to operate these institutions. For centuries, however, these specialized institutions were exclusively financial and judicial; this gave medieval states their peculiar character. After the institutions came changes in attitudes: theories of sovereignty among the intellectuals, and shifts of loyalty from family, local communities, and religious groups to the state. After 1450 there was little in the way of new institutional development. What the New Monarchies possessed that their immediate predecessors did not were time, money, energy, and subjects whose attitude toward the state had changed. "Possessing classes assisted rather than resisted their governments." New departments grew very slowly because their growth threatened the king's power of decision as much as it might at the same time have assisted him in making and executing those decisions.

Although the essay has few footnotes, readers who know the literature of the last forty years will have no trouble recognizing the wealth of articles and monographs that hide behind each sentence. The student or casual reader, on the other hand, will find it all disarmingly simple. The demanding reader, indeed, may find it too simple. The narration is so smooth, the highway so well paved, that all the ambiguities and—unfortunately—all the problems inherent in the subject have vanished; "unfortunately," because few medievalists are better placed than Strayer to know what the problems are. It is regrettable that he did not choose on this occasion to share that knowledge with a wider audience.

Among the problematic subjects that disappear from sight is the role of the Church in the development of the medieval state. How strange, for example, in a long paragraph devoted to representative institutions (pp. 64–66) to find no mention of councils and synods, and no mention of canon law (though Roman, feudal, and customary laws find their place) when he speaks of *Quod omnes tangit*. . . . To be sure the Church did not develop into a modern state, but it faced many of the problems of other thirteenth-century governments, and it contributed both ideologically and administratively to their attempted

solution. Strayer's views on the secularization of the thirteenth-century society, however, are well known. And at this point we perhaps begin to touch what is personal as well as scholarly in this essay.

Despite a disclaimer in the preface, the medieval origins of the modern state form, for Strayer, a success story. (This alone disqualifies the Church as part of a history of political organization.) As a historian Strayer clearly believes quite strongly in many of the values incarnate in the modern state. "Efficiency," "waste of human resources," "normal development"—when these phrases appear in the text they are not descriptions, they are normative evaluations. Strayer does not shy away from judgments when he feels they are required. These judgments, however, are in the deepest sense political rather than moral, and like the scholarship, they summarize a generation. The values that shape this essay are essentially the liberal political values of the generation from FDR to Lyndon Johnson. Their striking appearance here gives this little book an individuality that few modern books on medieval history can claim.

Thinking back to Charles Petit-Dutaillis and Robert Fawtier, and closer to current controversies, I puzzled to myself how far we have come in so short a time.

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Amherst College

ROBERT BOUTRUCHE. *Seigneurie et féodalité: L'apogée (XI^e–XIII^e siècles)*. (Collection historique.) Paris: Aubier. 1970. Pp. 549.

The first volume of this work was published ten years ago. It gave promise then of becoming a major contribution to the historiography of the subject (reviewed *AHR*, 66 [1960–61]: 713–14), and now this second volume realizes much of that promise. The project, however, has grown, and the treatment of the decline of feudalism is reserved for yet another volume. Here Professor Boutruche finds it quite enough to deal with the high period of manorialism and feudalism, the eleventh to thirteenth centuries. Sometimes called the second period of feudalism, it is the age of classical institutions of lordship both over vassals and over serfs. Growing out of the localism of the

tenth century, feudalism and manorialism provided the leadership and ties of dependence that largely pulled Western Europe together again politically. The landscape was altered fundamentally with the great clearing of the forests and the building of castles across the face of Europe. The manor became more efficient in the exploitation of the land and more complex in its political relationships. Feudalism also became more complex and developed its contractual system of government consecrated in customary law.

What is most striking about the work is its use of the regional studies of the past thirty years to show how feudalism differed from place to place as well as from time to time. No longer is it sufficient to differentiate between Germany and France, for example; one must recognize the provincial differences within each country, the nearly infinite variations from lordship to lordship. Conversely, one must recognize the similarities of Norman feudalism, whether it be in Normandy, England, the Two Sicilies, or the principality of Antioch in the Holy Land. Far greater depth and certainty are possible in the study of these institutions than heretofore, if much less uniformity and simplicity. In this work the north of France provides a norm from which other countries vary in greater or lesser degree. One may wonder if this method is quite just, but it undoubtedly provides unity of perspective. There can be no question that Professor Boutruche has mastered his subject in all its diversity: his bibliography comprises nearly a thousand titles and is a contribution to scholarship in itself. His footnotes are crowded with illustrative details. A ninety-page appendix of documents in translation provides interesting exemplification.

In a work of this size and complexity a reader will naturally have questions. Is there any reason to separate English "honors" from earldoms or baronies (pp. 263-64)? Had the honor of Chester anything to do with protecting the Scots border (p. 259)? How widespread was fifteen years as the age of majority (pp. 226-28)? What evidence is there for ducal fiefs in Normandy before 1050 (p. 332)? Were the ducal limitations on Norman feudalism "foreign to vassalic principles" (p. 316)? Has feudalism "centrifugal forces" (p. 251)? This last

suggestion seems to contradict the whole tenor of the work, to hark back to the earlier view of feudalism that Professor Boutruche regards as unjust and derogatory. It suggests the slow progress we make in revising historical judgments. The great value of this work is that it goes so far to present to a wider audience the revisions that have been made in the specialized literature. All of us will look forward with certain profit to the publication of the final volume of the work.

FRED A. CAZEL, JR.
University of Connecticut

GIUSEPPE ALBERIGO. *Cardinalato e collegialità: Studi sull'ecclesiologia tra l'XI e il XIV secolo.* (Istituto per le Scienze religiose di Bologna. Testi e ricerche di Scienze religiose, Number 5.) Florence: Vallecchi Editore. 1969. Pp. viii, 220.

Professor Alberigo has explored an area of church history and theory curiously neglected by earlier writers. His style is lucid, and his conclusions are based on the sources and the abundant recent literature on medieval ecclesiology.

Alberigo's work is not concerned with the practical working of the college of cardinals as an institution but with the development of the theory or doctrine of the cardinalate, known to late medieval authors as *ius divinum cardinalatus*. The importance of the cardinals of the Roman Church vastly increased with the Gregorian reform in the eleventh century, the consequent centralization of the Latin Church round the papacy, and the decline in importance of the diocesan bishops. As the bishops lost their position as a corporate body, the cardinals ceased to be essentially assistants to the bishop of Rome in his liturgical functions and assumed the role that bishops had previously enjoyed in the government of the universal church.

Alberigo discusses the decree of 1059 regulating papal elections and analyzes the views of the eleventh-century reformers, for whom the pope and cardinals formed indissoluble parts of an organic whole, the Roman Church. Alberigo then discusses St. Bernard on the cardinalate, the *Per venerabilem* of Innocent III, with its derivation of the college from the Levitical priests of the Old Testament, and

the growth of the thesis that the cardinals were the successors of the apostles. The analysis of the position of Hostiensis is particularly notable. Chapters 3 and 4 deal with fourteenth-century controversies and canonists and with the beginnings of the Great Schism. The theory of the "divine right" of the college, attacked by Occam, began to decline with the postconciliar papacy. After the Council of Trent the power of the cardinals was reduced to the election of the pope. Vatican II seems to have reversed the developments of the eleventh century and to have given back to the episcopate—at least in theory—the theological position the cardinalate enjoyed for so many centuries.

J. N. HILLGARTH
Boston College

FRANK BARLOW. *Edward the Confessor*. Berkeley and Los Angeles: University of California Press. 1970. Pp. xxviii, 375. \$10.95.

Although sprightly and clearly written and grounded upon a remarkable knowledge of the sources and scholarly literature, this history of Edward the Confessor, the latest in the series initiated by D. C. Douglas on the English monarchs, still leaves unresolved whether a series devoted to the lives of England's medieval kings makes much sense. Professor Barlow has laboriously pieced together all the bits and scraps on Edward that are ever likely to be found; but are the results worth the effort? Except for some corrections and some revised attitudes vis-à-vis the eleventh century, the story of Edward remains unchanged. Perhaps, as Barlow argues, Edward was a more able warrior, a more astute politician, and a more efficient administrator than we have thought, but this argument derives from personal opinion and not from the sources. This erudite volume simply reinforces what is already known: that Queen Edith, Godwin, Harold, Stigand, and others exerted great influence and power over Edward, that he seldom did any of the fighting, that his lack of heirs produced the duel between Harold and William of Normandy, and that his thoughts on the succession are really unknown.

Sandwiched between six chapters on Edward's life and reign to 1053 and three on the

remainder of his reign are two on the royal government. A final chapter telling how Edward became a saint is followed by appendixes on scholarly problems, lists of rulers and dates, and some good maps. The best chapters are those on the government and the canonization, probably because for them Barlow had enough evidence to make definite conclusions. Information gathered from charters and writs plus a little from the narrative records indicates that English government, both central and local, was reasonably efficient and not in total disarray. Of interest for the diplomatist are the sections on Edward's royal titles and the edition of some of his charters; and for the numismatist the pages concerning royal policy on coinage. The most intriguing story revolves around the campaign of Osbert of Clare, sometimes and sometimes-not monk of Westminster Abbey, to secure canonization for Edward. It is a tale of perseverance and calculated maneuvering, a testament to the power of propaganda and medieval credulity. That a man with as few cures to his credit as Edward should have achieved sainthood in 1161 must be considered a remarkable feat even in the Middle Ages. But, as Barlow notes, most of the principal actors also gained something; the chapter of Westminster, a valuable cult; Henry II, a holy monarchy; and Becket, an idea of how to become a saint.

But we still ask whether medieval royal biographies are of much value? Unlike Freeman and numerous predecessors, Barlow has labored "to reconstruct Edward in his contemporary setting" (p. xx), but with little success because, as he admits, "we have to scrape the barrel with care: every scrap of information is precious" (p. xxvii). And later he writes: "The second decade of Edward's reign is more obscure than the first. The narrative sources almost dry up and the charter evidence nearly fails" (p. 188). Such statements emphasize the problem and show why Barlow cannot extricate himself from contradiction. Concluding first that the place of Edward in English history is "not easy to define" and that "he made no great impression on his contemporaries," Barlow then states that close analysis of Edward's problems and behavior "reveals his intelligence and resourcefulness if not good judgment and wisdom" (p. 286). And how de-

fensible is the statement that Edward's "real achievement is often ignored. His kingdom passed entire to Harold and then to William; and that kingdom he had helped to fashion" (p. 287)? What seems more realistic and truer to contemporary observation is that the realm went to Harold because he was the most powerful, and that he lost it because he was defeated at Hastings. These events had no connection with what Edward may have done or planned.

Professor Barlow, indeed, had a difficult assignment. How could he hope to make Edward emerge from the medieval murk as a real man with emotions, drives, and motivations when others, working with far better sources, have had little more luck in breathing life into Charlemagne, Henry II, and Saint Louis? Instead of elusive biography, would it not be more sensible for medievalists to concentrate their talents upon problems for which documentation affords them at least a fighting chance to practice their profession of historian?

BRYCE LYON
Brown University

A. R. MYERS, editor. *English Historical Documents, 1327-1485*. (English Historical Documents, Volume 4.) New York: Oxford University Press. 1969. Pp. lxvii, 1236, 5 tables. \$29.50.

Enormous effort has gone into the preparation of this latest volume, somewhat longer than its predecessors, in the English Historical Documents series. The material is arranged in four parts: "The Political Framework," "The Government of the Realm," "The Church and Education," and "Economic and Social Developments." The editor has surveyed recent scholarship in separate introductions for each part and has provided extremely thorough bibliographies of printed original and secondary literature.

Occasionally one may question some of Professor Myers' statements. The Salic law did not operate in fourteenth-century France (pp. 8-9). It is unwise to ascribe an important role to Margaret of Anjou within the first five years or so of her arrival in England (p. 26), when our information on this comes from Tudor sources. A list of periods during which the

chancellor was not a bishop is incomplete (p. 370). The discussion of the economy in the thirteenth and early fourteenth centuries relies heavily on views that have been criticized and have yet to be thoroughly tested (p. 926).

But lapses such as these are not serious in so comprehensive a work. Some of the introductory sections, moreover, bring together the results of research that is often neglected in general accounts of the period—that on the administration of the Church, for example. On the other hand, it is difficult to avoid feeling that the labor that went into the introductions was misplaced. Both the size and the price of this book mean that it will have few purchasers outside libraries and that those who use it there will do so for its documents and bibliographies. The introductions will not secure the attention that the labor and learning devoted to them deserve.

Every specialist in the period will have his own views about the choice of documents in this work, and I must admit that I am, in effect, asking for more. But some insight should have been given into the campaign of 1359 and the negotiations that led to the Treaty of Brétigny (doc. 39). Why not give a portion of the St. Albans account of the Good Parliament side by side with that of the Anonimale chronicle (doc. 46)? Can an account of the Percys' rebellion of 1403 (doc. 88) be satisfactory without an excerpt from Hardyng? It is surely more important to reproduce the charges against the duke of Suffolk in 1250 than the account of his death (doc. 144). It is a pity that some manorila materials are not printed to show in detail the effects of the Black Death. A few more indentures of retinue could have been provided to illustrate the development of this form of agreement between lord and man. The weakest selection is that dealing with the law (pt. 2, C), where more use could have been made of the often fascinating interplay between doctrine and procedure displayed in the Year Books.

J. M. W. BEAN
Columbia University

IRENE HASELBACH. *Aufstieg und Herrschaft der Karlinger in der Darstellung der sogenannten Annales Mettenses priores: Ein Beitrag zur Geschichte der politischen Ideen im Reiche*

Karls des Grossen. (Historische Studien, Number 412.) Lübeck: Matthiesen Verlag. 1970. Pp. 208. DM 28.60.

This study attempts to illuminate the political mentality of the Carolingian age by analyzing a single document, the enigmatic *Annales Mettenses priores*. The results are well worth the attention of Carolingian specialists.

The first two chapters are devoted to the genesis of the *Annales*, a task made necessary by the shifting viewpoints, not always compatible, reflected in the document. Working from previous scholarship that has established that the *Annales* were the work of a single author writing about 805, Irene Haselbach seeks to demonstrate that the compiler repeatedly changed his viewpoint and his literary genre and continually reworked earlier parts of his history as the work evolved. This constantly shifting focus resulted from fresh issues suggested by the events surrounding the Carolingian rise to domination, issues of particular concern in the political climate in which the annalist worked just after Charlemagne's imperial coronation.

Satisfied that her explanation of the construction of the *Annales* establishes the political consciousness of its compiler, the author then proceeds to a detailed analysis of the treatment given in the *Annales* to various phases of Carolingian history between 687 and 805, especially events down to the elevation of Pepin I to the kingship and the emergence of his Italian policy, and to several crucial political concepts, including the nationhood of the Franks, the relationship between ruler and nobles, and the Christian vision of the state and its governance. Her discussion—too complex to treat in a short review—is painstaking, faithful to the sources, cognizant of modern scholarship, and generally convincing.

On the basis of her study the author reaches certain conclusions about the political concepts reflected in the *Annales*. The work seeks to legitimize the rise of the Carolingians on the basis of a mixture of concepts derived from ancient Germanic, Frankish, and Christian sources: the *Geblütsheiligkeit* of the Carolingian family, the divine favor extended to individual rulers whose conduct was pleasing to God, the Carolingians as the elect of the Frankish *gens* and especially its nobles, and

the establishment of Frankish overlordship of other peoples. The annalist appears especially intent on establishing the idea that the Carolingians exercised real power long before they acquired the titles that went with power—even the imperial title that came in 800. By justifying the Carolingians on these grounds the annalist reveals himself as a conservative, standing in contrast with the “modern” ideas of an Alcuin, who placed greater stock in propagandizing patristic concepts of Christian universalism emanating from Rome. The author believes that the annalist was trying to justify the emperorship of Charlemagne to a troubled segment of the Frankish nobility on the basis of traditional political concepts, dating chiefly from the era of King Pepin I, while resisting the intrusion of Roman ideology and its chief proponent, the papacy. This analysis of the *Annales Mettenses priores* makes considerable sense.

RICHARD E. SULLIVAN
Michigan State University

JOHN W. BALDWIN. *Masters, Princes, and Merchants: The Social Views of Peter the Chanter & His Circle*. Volume 1, *Text*; Volume 2, *Notes*. Princeton: Princeton University Press. 1970. Pp. xvi, 343; xi, 287. \$22.50 the set.

Though he no doubt wishes them well, Mr. Baldwin has parted company in this book with the new historical technologists. It took strong convictions for him to take a stand in the company of A. Luchaire because, like G. G. Coulton, he wished “to get at the real Middle Ages” through studying “genuine human documents.” He foreswears to imitate numerologies and analytic inventions of more recent scholars. And yet the book is a rich banquet to which all are welcome—even statisticians, demographers, and sociologists.

The core of Mr. Baldwin's study was a set of lectures and disputations that he came across in the Bibliothèque Nationale nearly twenty years ago. Those texts were composed by men active in theological studies at Paris in the late twelfth century, and Peter the Chanter seemed to be the central figure. Mr. Baldwin has not provided an inventory of the manuscripts he discovered. The notes, which occupy a separate volume, clearly show that he en-

larged his original corpus of texts with further discoveries in French and English archives. Furthermore, from such clauses as "one may assume," "one may surmise," and "in all likelihood," I conclude that the "circle" of Peter the Chanter was not a coterie of scholars, but a network of rapports, some personal and others wholly literary, in which the Chanter played influential parts. The author explains these connections in meticulous biographical sketches of Peter and the other leading figures of the study.

The texts under discussion were precise analyses of isolated, practical matters, instead of systematic treatises on broad principles. Mr. Baldwin's task was to knit this fragmentary and particular evidence into a coherent scheme. He organized the material topically, putting into four separate sections views on theology and the learned world, on the powers and duties of government, on practical morality in commerce, and, finally, on Church order. The actual variety of information within these broad categories is extremely wide. To draw his texts into an expository scheme, Mr. Baldwin embedded citations from them in an ample matrix of historical narrative and collateral quotations from contemporary narrative and canonistic writings. Parts of his exposition are necessarily familiar: the bifurcation of theology and philosophy, for example. But the special value of Mr. Baldwin's work lies in the vast scope of the information that he opens to us and in the syntactic skill with which he convinces the reader that, despite the fragmented nature of his authors' views, "their writings provide a brilliant mosaic for the social life of their times."

KARL F. MORRISON
University of Chicago

JOSEPH R. STRAYER. *Les gens de justice du Languedoc sous Philippe le Bel*. (Cahiers de l'Association Marc Bloch de Toulouse. Études d'histoire méridionale, Number 5.) Toulouse: the Association. 1970. Pp. 211. 21 fr.

Structured in a somewhat unusual way, this book has 50 pages of text followed by a 150-page appendix. The appendix comprises short biographical notes on some 420 royal seneschals, judges, *avocats*, and *procureurs* who held office in the five seneschalships of the Midi

between 1280 and 1320. It forms the main part of the book, and it is preceded by an introduction in which Professor Strayer argues that the legists as a whole, particularly those skilled in Roman law, may often have hampered, rather than helped and encouraged, the growth of royal power in thirteenth- and fourteenth-century France. He thus offers a useful corrective to the too easily made assumption that, because a handful of legists in the central government significantly affected royal policies and contributed to the extension of royal authority, lawyers and the Roman law as a whole were weapons in the royal armory.

The biographical material set out here, though indexed only under Christian names, is invaluable, and certain broad conclusions that emerge from it are well brought out in the introductory pages, where Strayer shows, for example, that royal legal posts were poorly paid and none too popular and that private legal practice was much more rewarding. It was fees from private practice that enabled some of these lawyers to purchase land for themselves. Most of them were natives of Languedoc, bourgeois in background, and little interested in pursuing careers at Paris or elsewhere. Needless to say, the book is well organized, the material clearly set out, and the scholarship meticulous.

RICHARD VAUGHAN
University of Hull,
England

SHIRLEY NEILSEN BLUM. *Early Netherlandish Triptychs: A Study in Patronage*. (California Studies in the History of Art, Number 13.) Berkeley and Los Angeles: University of California Press. 1969. Pp. xv, 176, 80 plates. \$30.00.

This study fills a need in the literature of the history of art; its subtitle, *A Study in Patronage*, suggests the particular approach the author has chosen. Her detailed discussion of eleven fifteenth-century triptychs for which there is relatively complete documentation stresses the total context in which these works were created: the identification of the patron and his reasons for commissioning the work; the intended location of the altarpiece; the choice of subject and the artist's handling of it in the light of all these factors. For the first

time such a thorough historical treatment is integrated with a careful analysis of the works in relation to each artist's *oeuvre* and his place in the development of Northern art.

The author begins with an analysis of the economic and artistic milieu in which the triptych form developed and of its roots in architecture and sculpture. In the earliest altarpieces the donor and artist collaborated in working out an appropriate scheme for that donor's devotional purposes. The multiple fields of these three-panel hinged altarpieces, with their interior and exterior surfaces, were unified by a complex organization of scenes that stressed the religious content and the donor's devotion. Artists such as Jan van Eyck and Robert Campin invented new means of interpretation and made use of secondary meanings and a pervasive religious symbolism disguised in the naturalistically portrayed elements of the real world.

In chapters on eleven triptychs dating from the 1440s to the turn of the century, the author then documents the factors that attest to the gradual dissolution of that unity found in the earlier works. The close ties between artist and donor and the involvement of the artist in the interpretation of the religious content are weakened. From the 1450s, when scenes constructed on the basis of focal-point perspective appear in the works of Petrus Christus, the artist's primary allegiance shifts to the pictorial aspects of his pictures, and the multiple panels of the triptych form become an obstacle to creating the visual unity demanded by this approach. The conflict appears first in the works of Dieric Bouts in the 1460s and is evident even in the later altarpiece by Hugo van der Goes, who returned to the use of disguised symbolism in a very personal treatment of a traditional subject, the Nativity. Three triptychs by Hans Memling and one by Gerard David bring us to the end of the period in which this form had been, for a time, a meaningful one.

Detailed information on patrons such as Peter Bladelin and Nicolas Rolin, both in the service of Philip the Good, and the foreigners Tommaso Portinari and the Greverade brothers, provides us not only with a deeper understanding of the interaction between donor and artist but also with fascinating information on

the business and artistic relationships between Flanders and other countries.

The author adds new information to the wealth of scholarly material already available and contributes important reinterpretations as well as new suggestions for some persistent iconographical problems. The conclusions drawn from the material are generally carefully considered and supported by the evidence. Minor exceptions appear in two areas. More restraint should be shown in conclusions drawn from the idea that the donors are included as participants within three altarpieces, the *Last Judgment* by Rogier van der Weyden and the two by Dieric Bouts, for, as stated by the author herself, this is still conjectural and admittedly unsupported by any real evidence. The author states, too, that Petrus Christus was the only early Netherlandish master who was little interested in the triptych form, attributing this to his concern for representing unified spatial settings according to the laws of mathematical perspective. Yet, among works long attributed to Christus, a number of panels suggest that they once formed parts of triptychs, and in the last year (after publication of this book) two triptychs by the artist have been convincingly reconstructed from existing works.

The book is handsomely produced with welcome color plates and additional black and white illustrations of the works and related material. A small omission in an otherwise comprehensive presentation of the material is the failure to provide specific measurements for each work, although general references are made to size as a factor in the total effect. This is a minor flaw in an immensely valuable book that presents well-known material in a new light.

LOLA B. GELLMAN
Queens College,
City University of New York

CHARLES TRINKAUS. *In Our Image and Likeness: Humanity and Divinity in Italian Humanist Thought*. In two volumes. [Chicago:] University of Chicago Press. 1970. Pp. xxvii, 457; viii, 461-985. \$22.50 the set.

This is not a survey, as one might expect from its length, but a complex and subtle argument for a better understanding of the humanists'

goals. Dr. Trinkaus concentrates on selected texts of Petrarch, Salutati, Valla, Manetti, and Ficino, with less attention to about a dozen others. He presents long excerpts (the Latin text is given in the notes) with interpretive connecting summary to develop these writers' positions on the relationship of man to God and on the character of man in the frame of that relationship.

We begin with Petrarch's revulsion from late scholastic nominalism and his concern for inner religious experience, and move through Salutati's discussions of will and Valla's extraordinary mastery of classical ethical systems, and examine in later writers the dignity of man in a context of human frustration and misery. Emphasis on the distresses and motives of the writers in earlier chapters shifts toward more specific and controversial topics later in the book. The fourth and last part takes up the humanist's reading of Scripture and the rationale of their views on such matters as translation and the retention of medieval traditions in interpretation, along with their work on the sacraments, topics that have never been easy to relate to later religious history; their attitudes toward the professed religious, on which subject Dr. Trinkaus has done much to offset a great deal of hyperbolic history writing; on Platonism, a chapter (15) much illuminated by the earlier discussion of Ficino (chapter 9); and, finally, their efforts to reconcile what they could of competing religious systems, a chapter (16) that is most helpful to those curious about the later development of history and other literary forms.

This treatment gives new force and clarity to many familiar conclusions of Renaissance historiography, as well as to others that ought to be much more familiar than they are. The humanists were professional writers, whatever other jobs they may have had, and they were devout Catholic Christians. For them to write about religion, then, was to find their largest audience, but what appears most clearly was their intense personal concern with theological matters. If their work "revived" classical literature, such a thing came about through their reading of the Fathers, among whose sources the pagan writers figured large.

Dr. Trinkaus meets particular scholarly issues, for the most part, in his later chapters,

and then usually in notes: valuable as these are, they appear as incidental to his documentation, for the book is not polemical in character. If Hans Baron and his school find apparent contradictions in the utterance of "republican" sentiments by the likes of Salutati, Dr. Trinkaus can argue that theological concerns have been too much ignored in our historiography: what is consistent in theology may appear otherwise in a modern sociopolitical appraisal (p. 668 n.72). Nor can modern theological language always save us from confusion: we may characterize humanism by its search for "inwardness" in religious experience, but we cannot reduce a writer like Manetti to such a polarity as "immanence" against "transcendence" (chapter 6).

Dr. Trinkaus' prose is careful and rewards close attention, though he needs rather more space to make himself clear than does, say, P. O. Kristeller. There is a fine apparatus, especially useful in its discussion of manuscript materials. Translations lean toward the literal but remain in accord with sense. One does wonder, however, when all is done, whether the student who is willing to give this study the attention it deserves ought not to have the complete texts themselves in front of him—with Dr. Trinkaus' copious annotations. From this full treatment to the presentation of a corpus seems not too great a step to take, and the book itself is an argument for it—and for its own redundancy. But we are well ahead with this work before us as it is.

Valuable as it is, this book exposes a dilemma that it cannot solve. One of Dr. Trinkaus' many cogent points is that the world gave up the humanists' search for unity and that this change explains the misinterpretation of the Renaissance by both secular and religious historians (p. 764). But has not that search for unity become the business of scientists and their few philosophers? Few will read theology, or even about theology, today: is it because the problems the humanists discussed as a matter of what man was can only be presented now as a matter of what man knows? As theologians these humanists will stay in the attic indefinitely, and the scholar who brings them down to join us must be as conversant with the twentieth-century terms of their concern as Dr. Trinkaus has shown himself to be with

their own idiom—as well as with that idiom itself.

AVERY ANDREWS

George Washington University

DOMENICO MAFFEI, editor. *Enea Silvio Piccolomini, Papa Pio II: Atti del convegno per il quinto centenario della morte, e altri scritti*. Siena: Accademia Senese degli Intronati. 1968. Pp. xi, 455.

This handsome volume appears under the auspices of the Sienese Academy of the Intronati, but it is clear that interest has been taken in it by a number of other Sienese institutions, for example, the university and the bank called Monte dei Paschi. That devoted Sienese, Enea Silvio Piccolomini (1405–64), better known as Pope Pius II (1458–64), would have appreciated this tribute though he would have thought it strange that no article is in Latin. Sixteen scholars have contributed articles, some long and some quite short. Almost half the text is in Italian—really more than half, because a German article by Berthe Widmer on Piccolomini and the Swiss has been put into Italian. German is represented by the work of Alfred A. Strnad. English and French are balanced with fifty pages to fifty-one. It is a pity that no Czechoslovakian scholar has joined these sixteen with, for example, an assessment of Piccolomini's contribution to Bohemian history or an account of his conflicts with individual Hussite priests or with their "heretic king," George Podiebrad. There are forty-eight plates, six of them in color. The printer, a firm in Varese, is guilty of very few misprints.

Most of the authors have provided copious footnotes, and these are truly a mine of information. One learns, for example, that Remo Ceserani, who contributes (in Italian) "Notes on Pius II's Activity as a Writer," is preparing a revised critical edition of Vatican MS Reginensis Lat. 1995, which contains (in Latin) "The Commentaries of Pope Pius II." This one learns not from Ceserani's own article but from Ruth Olitsky Rubinstein's "Pius II's Piazza S. Pietro and St. Andrew's Head" in a footnote on page 222. Despite the utility of the Gragg-Gabel translation with notes, the publication of the actual text was a consummation devoutly to be wished for. It is to be

hoped that it will contain a full index of names.

It has seemed best to reserve anything like detailed comment to one or two of the longer articles, and I may perhaps be forgiven for beginning with Rino Avesani's "Epaenetorum ad Pium II Pont. Max. libri V," because a decade or more ago I had the idea of writing a similar article and have painfully assembled a good deal of material. It is a satisfaction of a sort that not even with the resources of the Vatican Library has Avesani excelled me on the poet with the unlikely name of Galassius Pauper. We are equal, each of us knowing nothing about him save his poem among the *Epaenetica*. Avesani also mentions seven other poets who contributed to this collection, of whom little or nothing is known (p. 10).

So far as is known there are only two MSS of the *Epaenetica* in existence, one in the Chigi collection in the Vatican Library, the other in the Biblioteca Comunale of Trieste. A number of the poems have been published, but the *Epaenetica* as a whole has not. Avesani accepts, as I cannot, Campana's thesis that T, the Trieste MS, is a copy of C, the Chigi one. Yet he points out that T lacks three poems that C has. They are by A. Tridento of Parma and together make up four hundred and five lines, or just about twelve pages at twenty-six lines to the page. Avesani also states that T lacks two rubrics that C has. He might have added that C lacks the heading to Nicholas de Valle's *Roma Constantinopoli sorori*, which T has. It has been added in the margin of C in a cursive hand. He has noted that the gatherings of T have been bound in a false order but not that a hendecasyllabic poem by Io. Antonius Campanus is completed on f. 131r (see p. 64). Avesani also accepts Cugnoni's assignment to Pope Pius II himself of the poem beginning *Hactenus aetherias claves*, which in both C and T is unheaded. As he gives no facts of his own in support, though he rightly classes this as one of the better poems, he has not shaken my arguments against the pope's being the author of the poem ("Pope Pius II's Use of Turkish Atrocities," *Southwestern Social Science Quarterly*, 46 [1966]: 407–15).

A reviewer in the *New York Times*, January 19, 1971, was of the opinion that more is

known about Pius II than of any other man of his day. If challenged he might have referred to the footnotes of "Studia piccolomineana," Strnad's long article. The plural *studia* is justified because Strnad begins with the somewhat familiar account of the humanists around Pius II, then deals with his books and those who copied or adorned them, and then gives what is in effect the life of Francesco Todeschini-Piccolomini, the nephew whose first steps towards an eventual brief pontificate as Pius III were his nomination as archbishop of Siena and his promotion to cardinal, both brought about by Pius II. This biography is systematic until the death of the uncle when the nephew was twenty-five but thereafter is more episodic, centering on such things as his books, his palace in Rome, and the famous Libreria in the Cathedral of Siena.

Cecil H. Clough's "The Chancery-files of Aeneas Silvius Piccolomini" is a plea for the publication in calendar form of whatever of these have survived. The names of the other contributors to this valuable *Festschrift* are Franz Babinger, Gino Franceschini, Franco Gaeta, Myron P. Gilmore, Guido Kisch, Charles Lefebvre, Gioacchino Paparelli, José Ruyschaert, Antonio Stäuble, and Giulio C. Zimolo.

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KARL H. SCHWEBEL. *Der Stralsunder Friede (1370) im Spiegel der historischen Literatur: Eine Übersicht.* (Schriften der Wittheit zu Bremen. Jahrbuch der Wittheit zu Bremen, Volume 14.) Bremen: Verlag Friedrich Röver. 1970. Pp. 232.

The year 1970 was a double anniversary. The peace that concluded the second war between King Waldemar IV of Denmark and the united forces of some Hanseatic and other north German cities was signed on May 24, 1370. And it was a hundred years ago that, on the quincentenary of the Peace of Stralsund, the Hanseatischer Geschichtsverein was founded. At that time the Hanseatic towns celebrated their past glories with pompous patriotism, as witnessed by the prize-winning euology written by Dietrich Schäfer (*Die Hansestädte und König Waldemar von Dänemark* [Jena, 1879]).

This time the tone is a detached and calm examination of past and present prejudices. And today's "festive" publication is the modest historiographic essay of Dr. Schwebel.

The author investigated the literature on the Peace of Stralsund, contemporary sources, and later authors up to our day. He uses them as evidence of understanding as well as misunderstanding of a single momentous event. The results of his exhaustive survey contain no surprises. The contemporaries commented on the Peace only to the extent of their immediate concern. The succeeding centuries witnessed a slow progress toward better information and evaluation, repeating, however, historical errors often with astounding tenacity. During the last period, nineteenth- and twentieth-century writings reveal a growth of factual knowledge, of critical analysis but also of conflicting nationalistic bias.

The survey is organized by historical territories. Beginning with Lübeck, Hamburg, and Bremen, the author proceeds through northern Germany up the Baltic coast to the Scandinavian countries and concludes with the northern Netherlands. Each chapter opens with an account of fourteenth- and fifteenth-century chroniclers, followed by early modern authors of the sixteenth through eighteenth centuries, and lastly by historians of the nineteenth and twentieth centuries. A final chapter covers general Hanseatic history, where the only American author, D. K. Bjorn, and his article in *Speculum*, volume 7, in 1932 are mentioned. The conclusion quotes a few added excerpts from encyclopedias and general histories. Hardly anything on the subject could have been overlooked.

In analyzing this rich material, the author is at his best assessing the methods of the chroniclers and early historians. The modern period with its methodical and ideological complexity is treated rather superficially and pedantically. He is sincere and very vocal indeed in putting nineteenth-century estimates of Germanic greatness in a more sober perspective. An annotated bibliography might have been handier, though that would have deprived the Wittheit zu Bremen and the author—its vice-president—of their earnest disclaimers of all that pomp and circumstance

of 1870. Still, a good index of names and titles would have been useful.

JANOS M. BAK

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JÖRG FÜCHTNER. *Die Bündnisse der Bodenseestädte bis zum Jahre 1390: Ein Beitrag zur Geschichte des Einungswesens, der Landfriedenswahrung und der Rechtsstellung der Reichsstädte.* (Veröffentlichungen des Max-Planck-Instituts für Geschichte, Number 8.) Göttingen: Vandenhoeck & Ruprecht. 1970. Pp. 367. DM 37.

PETER EITEL. *Die oberschwäbischen Reichsstädte im Zeitalter der Zunfttherrschaft: Untersuchungen zu ihrer politischen und sozialen Struktur unter besonderer Berücksichtigung der Städte Lindau, Memmingen, Ravensburg und Überlingen.* (Schriften zur südwestdeutschen Landeskunde, Number 8.) Stuttgart: Müller & Gräff. 1970. Pp. xix, 321. DM 24.

Bodenseestädte were cities situated around Lake Constance: Constance, Zurich, St. Gall, Schaffhausen, Lindau, Überlingen, Ravensburg, and a few others. Joining with towns in the Upper Rhine and Swabian regions, but often comprising a separate group among them, they formed alliance organizations of self-help against infractions of their special status vis-à-vis the emperor and territorial princes. At first highly successful, the leagues came to grief in the *Städtekrieg* of the end of the fourteenth century, when territorial rulers led by Bavaria defeated the cities and imposed the Peace of Eger in 1389. Füchtner's volume, originally a Göttingen dissertation, tells the story from the point of view of the major cities of the lake region, emphasizing not so much the political events as the treaty documents themselves. All aspects of the many consecutive unions are examined in meticulous detail: shifting membership; military provisions and administration; revenues; the machinery for lodging complaints followed by an *Erkenntnisverfahren*—the formal investigation of circumstances—leading to the indictment of the malefactor; and the appointment of referees to deal with strife among and unrest within cities. Maintaining internal peace turned out to be the most important function of city leagues, which is why the emperor Wenceslas gave his approbation to the union of *Boden-*

seestädte once the defeat of 1389 had put an end to the cities' desire for autonomy.

It is good to see all this set out in its particulars, but it is not really very absorbing. As if to prove the superior interest of social over conventional political-diplomatic history, Peter Eitel examines some of the same cities at a slightly later time and from a different angle of vision: the distribution of wealth and political power among the cities' class and occupational groups. The inquiry moves within boundary dates suggested by constitutional changes occurring in all Swabian cities at roughly the same time. In the middle of the fourteenth century political power began to pass from a handful of patrician families to larger numbers and wider circles of burghers organized in craft guilds. Hence we speak of a period of *Zunfttherrschaft*, in which guild members exercised political power and that lasted from the 1340s to 1551–52, when Charles v, following his success in the Schmalkaldic war, imposed on the cities a reactionary, oligarchic constitutional system designed to minimize guild influence.

Eitel's study, which began as a Tübingen dissertation, utilizes archival sources from four cities where the documentary evidence suffices for statistical generalization: Lindau and Überlingen on Lake Constance, Memmingen and Ravensburg in Upper Swabia. Eitel uses lists of officeholders (given in a bulky appendix) and tax records. The results are set out in tables and graphs and are commented upon. Several interesting points emerge. In all the cities the number of the rich increased substantially in the period under discussion. Memmingen and Ravensburg especially, important trading centers both, boasted large numbers of sizable fortunes. In the latter city the ten richest families held 32 per cent of the total citizen fortune in 1497; the hundred richest held over 80 per cent. In grain and wine cities like Überlingen wealth was less concentrated. Most of the big money was in the hands of old patrician families, now organized in guilds as the law obliged them to be and continuing to play prominent roles in long-distance commerce as well as in city politics. As for the other guilds, a given occupation group's share of the general wealth differed from town to

town. In Ravensburg the "taylor" (including apothecaries, rope makers, and furriers) stood next to patricians in affluence; in Überlingen it was the bakers (including millers and salt merchants). When income (actually fortune) figures are coordinated with the lists of officeholders, it becomes evident that—not surprisingly—the greater the fortunes of the members of a given guild, the larger and more lasting that guild's influence in city politics. Individuals did not usually hold office repeatedly or for long stretches of time; the lists show constant turnover; new names keep appearing. But the same income groups predominate decade after decade. Everywhere the largest share of political power continued to go to patricians—not only as moneyed men, but also as persons of good connections abroad, wide experience, and training (but not academic education). But all officeholders, patrician or burgher, were well-to-do men; their fortunes were always considerably higher than those of average citizens. While the 1530s and 1540s saw some equalization of wealth (mostly through emigration of top families who began to prefer country estates to town dwellings), the political character of guild-controlled cities did not change until the calamities of 1551–52.

This is a useful book purveying interesting information; it is well written and clearly documented. The many tables, charts, and graphs are particularly valuable, and the bibliography is sound.

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CHRYSA A. MALTEZOU. 'Ο θέσμος τοῦ ἐν Κωνσταντινουπόλει Βενέτου Βαΐλου (1268–1453) [The Status of the Venetian Bailiff in Constantinople 1268–1453] (The Philosophical School of the National and Kapodistrian University of Athens: The Library of Sophia N. Saripolou. No. 6). Athens: the University. 1970. Pp. 251, 11 plates.

This is a serious book. There are excellent studies of the Venetian colony in Constantinople, and two lists of bailiffs (Venetian, *bailo*) exist; but the latter are incomplete and have no references, and the former say virtually nothing about the bailiff. Maltezou's book fills this important gap. The office of the Venetian bailiff in Constantinople made its appearance in 1268

and continued to exist down to the end of the Byzantine Empire in 1453. The bailiff's functions were twofold: to represent Venice in its dealings with the Byzantine government and to serve as the executive officer of the Venetian colony in Constantinople.

The first part of the book deals with the status of the bailiff as the representative of Venice in its dealings with Byzantine government (the bailiff does not quite yet have the character of a modern ambassador), the second with his status as the executive officer of the Venetian colony in Constantinople. The third part constitutes a list of all the bailiffs with the necessary discussion and references. The topics discussed range from the circumstances of the establishment of the office to the relations of the bailiff to the Jews of Constantinople and include the etymology of the term bailiff, the relations of the bailiff to the court of Constantinople, the mode of his designation, his salary, place of residence, and his family. Particularly interesting is the analysis of the structure of the colony and the role of the bailiff as the supervisor of its administrative setup; its religious, commercial, and, in general, economic life; and in the administration of justice. The picture drawn is precise and clear.

Appended to the main body of the book is the Latin document on which the study is based. The document was discovered by the author and is published here for the first time. There is also a facsimile reproduction and a summary in Greek of the contents of its various chapters. Whatever the bibliography of the role of Venice in the Near East may be, this is a book that no student of that role can afford to ignore.

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MODERN EUROPE

DONALD R. KELLEY. *Foundations of Modern Historical Scholarship: Language, Law, and History in the French Renaissance*. New York: Columbia University Press. 1970. Pp. x, 321. \$10.00.

NANCY S. STRUEVER. *The Language of History in the Renaissance: Rhetoric and Historical Con-*

sciousness in Florentine Humanism. Princeton: Princeton University Press, 1970. Pp. 212. \$6.95.

These two books are evidence of the recent growing interest in Renaissance historiography, particularly in the problems of the nature of the historical consciousness of the Italian and French humanists and the place of history in the general spectrum of their intellectual interests.

Mr. Kelley gives us an admirable statement of his assumptions. This study "rests upon the proposition that many of the important insights and breakthroughs in the interpretation of history were made not by the grand figures of historical narrative or of political philosophy but by pioneers laboring in relatively obscure and technical fields of scholarship and concerned more with the texture than the structure of history" (p. 13). Rejecting any transcendental or universal operating principle, he adopts a neutral definition of that ambiguous concept "historicism," referring it "to that cast of mind which, consciously or not, turns not to nature but to the world of man's making; which seeks out not the typical but the unique; which emphasizes the variety rather than the uniformity of human nature; which is interested less in similarities than in differences; and which is impressed not with permanence but with change" (p. 4).

With these premises Kelley traces the rise of the *studia humanitatis* in Italy, the new importance of grammar and rhetoric, and the emergence of philology, especially in the work of Lorenzo Valla. In his *Elegantiae* and in his legal and Biblical studies, Valla is rightly presented as a figure of revolutionary importance in the history of historical scholarship. From his innovations a direct line leads to the French scholars of the sixteenth century.

Most of Kelley's book is concerned with the great French *érudits* from Budé to Pasquier. He establishes Budé's place as the archphilologist whose encyclopedic mind embraced the study of Roman law, Roman coinage, the Greek language, and the institutions of the ancient world and of medieval and contemporary France. In the *De transitu Hellenismi ad Christianismum* of 1535 Kelley sees, perhaps unjustly, Budé yielding to a "transcendent impulse" and moving away from his earlier com-

mitment to a genuinely historical perspective. This commitment, however, dominated scholarship in sixteenth-century France, and Budé himself found a worthy continuator and biographer in Louis Le Roy.

Subsequent chapters are devoted to the very important historical school of Roman law inspired by the work of Budé but directly created by Andrea Alciato during the brief period of his teaching at Bourges. Among the followers of this school who represented one version or another of the *mos Gallicus* were Douaren, Baron, Hotman, Baudouin, and the greatest of all, Jacques Cujas, who more than any other single figure founded modern scholarship in the field of the history of law.

Subsequent chapters describe the work of François Baudouin in historiography; Charles Dumoulin in canon and feudal law; scholars like Du Haillan and the brothers Du Tillet, who devoted themselves to the medieval records of the French monarchy; and antiquaries like Pierre Pithou and Loisel. A century of scholarship of this type culminates in a certain sense in the work of Étienne Pasquier, whose *Recherches de la France* demonstrated the possibility of writing a comprehensive history of a civilization, illustrating the interconnections between cultural phenomena.

This study, modest in compass, careful in its scholarship, clear and convincing in its conclusions, is the best I have yet encountered on the subject.

Mr. Kelley refers throughout his analysis to the importance of the study of language for the beginnings of a historical consciousness. He finds that for both Budé and Pasquier it is in language that the substance of human reality is best revealed. This relationship is also the theme of Mrs. Struever's book, but there is an interesting difference on what kind of linguistic changes are most important for the historian to take account of. Whereas Kelley recognizes that changes in vocabulary constitute the most concrete evidence of historical change and therefore emphasizes the significance of philology, Mrs. Struever invokes a broader dimension and puts the accent on philosophy of language. She maintains that "it is in the area of philosophy of language not philology that the important humanist achievement lies" (p. 64). This prop-

osition leads her to find a starting point for the consideration of the humanists' philosophy of language in what she calls the rehabilitation in recent times of the Greek Sophists and their rhetoric, with particular attention to Gorgias. In a background chapter of breathtaking generalizations she surveys the relationship of philosophy and rhetoric from Gorgias to the Italian Renaissance. She concludes that "insofar as the Renaissance re-created the original Sophistic concepts and made them a possession of the modern Western intellectual tradition, it liberated historical consciousness from some major liabilities" (p. 37).

In three succeeding sections on rhetoric, poetics, and history; on rhetoric, politics, and history; and on rhetoric, ethics, and history (centered respectively on the works of Coluccio Salutati, Leonardo Bruni, and Poggio Bracciolini), Mrs. Struever proceeds to document her thesis. Insofar as she finds in rhetoric an orientation toward the realm of the possible, dealing with ambiguities and change rather than permanence, she is close to Mr. Kelley in her conception of historicism. In her specific illustrations of the relationship between rhetoric and history in the works of these three chancellors she often has illuminating comments to make. Some of her general statements have an abstract and even an a-historical character, however, that makes them difficult to follow and to accept. Mrs. Struever is very widely read not only in the literature on humanism but also in ancient and modern philosophy and linguistics, but in spite of some thought-provoking and challenging juxtapositions, I am not convinced that our understanding of fifteenth-century authors is deepened by applying to an analysis of their works categories derived from either Gorgias on the one hand or Heidegger on the other.

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HEINZ OTTO BURGER. *Renaissance—Humanismus—Reformation: Deutsche Literatur im europäischen Kontext*. (Frankfurter Beiträge zur Germanistik, Number 7.) Bad Homburg v.d.H.: Verlag Gehlen. 1969. Pp. 510. DM 38.

While English and American historians tend to relate Renaissance humanism to the Enlightenment, Heinz Otto Burger, whose early

work was in Swabian Romanticism and the esthetics of the *Meistersänger*, relates it to the period of *Sturm und Drang* and early Romanticism. Enea Silvio Piccolomini's *De Duobus Amantibus* (1444) is described as having an impact in Germany similar to that of Goethe's *Werther*; *De Praeclaris Picturae Professoribus* (1505) by Johann Butzbach, a monk of Maria Laach, is compared to Wilhelm Wackenroder's *Herzensergiessungen eines Kunstliebenden Klosterbruders*. Hence Burger's Renaissance is continuous with (rather than opposed to) the Middle Ages, and its uniqueness is defined in terms of feeling rather than in terms of rationalism.

Following Jean Leclercq, Burger argues that medieval monastic theologians anticipated the humanists both in their suspicion of mere *curiositas*—the scholastic learning that Petrarch, too, rejected as useless—and in their feeling for the beauty of language. Sigismund Gossembrot, a successful patrician turned monk, praised the study of the classics in 1466 in the same terms that monks had applied to meditation or spiritual reading—*ruminare, gustus interior*. Humanists turned to rhetoric because they sought an *ars movendi*, a means to touch the inmost heart.

Burger finds a *deutsche Eigenrenaissance*—a new affirmation of emotional life—in vernacular literature. *Der Ackermann aus Böhmen* (1401) vindicates both the joy of marriage and the grief of bereavement against a Stoic *apatheia*. Heinrich Wittenwiler, according to Burger, seemed fascinated by the crude vitality of the peasant life he satirized in *Der Ring* (1418). Somewhat later the young Mainz humanist Dietrich Gesemund praised the emotional release of Carnival in a dialogue, *Cum Catone Certomio de Furore Germanico* (1495). Meanwhile other humanists attacked Stoic morality by extolling *voluptas*. Conrad Celtis' *Amores* (1502), to which Burger devotes more space than any other humanist writing, celebrated both profane sexuality and cosmic *eros*. At a different level monks defined good conscience as the highest pleasure available to man, and the humanist-astronomer Johannes Regiomontanus described the sheer pleasure (*voluptas*) of intellectual inquiry, thus giving *curiositas* a positive meaning. Agrippa von Nettesheim, the magician whose reputation

may have been an element in the Faust legend, went so far as to describe *furor*—particularly the Saturnine *furor* that Ficino called *melancholia*—as the source of creativity, indeed the manifestation of man's inner divinity. In so doing he attained "the uttermost possibility of the humanist Renaissance."

Burger's argument is very suggestive at many points, and his book is further distinguished by an unusual command of American Renaissance scholarship (he taught at the University of Kansas, 1942–47, starting as a war prisoner). But his more general categories will not find wide acceptance. In treating rhetoric as a highly personal *ars movendi* he makes the fundamental error of confusing a novel intellectual technique with the traditional philosophical justification advanced by some of its proponents. As to his description of the Renaissance as a revival of emotional life, one would have to make comparisons with troubadour and *Minnesänger* romance (not, as Burger does, with their fifteenth-century *epigoni*) in order to establish that there is something really new about *Der Ackermann aus Böhmen* or *De Duobus Amantibus*. Finally, it is surely wrong to say that "every genuine humanist" rejected Stoic *apatheia*. The typical humanist was not an adventurous *Wandervogel* like Celtis but a harried classroom teacher whose views on emotional spontaneity were perhaps less than positive. It is true that Erasmus, to whom lesser humanists looked for guidance, specifically repudiated Stoic wisdom in *The Praise of Folly*. But Burger overlooks other and nearly contemporaneous works in which Erasmus applauds the Stoics for their brave effort to subject foolish passion to strict control by reason. One must view humanism not as a preview of Romanticism, or of the Enlightenment, but rather as a protean mingling of intellectual tendencies that would only later diverge.

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OTTO VON HABSBURG. *Charles V*. Translated from the French by MICHAEL ROSS. New York: Praeger Publishers. 1970. Pp. xiv. 258. \$8.95.

The Emperor Charles v, lord of nearly all of Western Christendom by multinational herit-

age, elected office, and conquest, has long been an object of historical controversy. Earlier and more extreme partisan views gave way in the later nineteenth and early twentieth centuries to the sober assessments, buttressed by a wealth of newly accessible archival material, of, among others, Modesto Lafuente, Walter Friedensburg, Karl Brandi, and Roger B. Merriman. Their image of Charles was on the whole that of a humane prince with many attractive traits, a prince who, however, in Merriman's words, led Spain down "the path of reaction rather than of progress, of darkness rather than of light." But lately an increasing number of historians tend to agree uncritically with Charles v's own side of his story, and in addition they project on this cosmopolitan ruler a sort of wish fulfillment of their rather nationalistic dreams. Some Spaniards hail him as the founder of *hispanidad*. German writers such as Peter Rassow, Carl Burckhardt, Gertrude von Schwarzenfeld, and the Freiherr von der Heydte regard Charles v with an amazing veneration, seeing him as a near-holy model for a new leadership of a utopian Europe, pan-Catholic and unified presumably under German auspices.

Otto von Habsburg bases his short book about his most famous ancestor chiefly on recent publications by this hagiographic school. The author, a political scientist and himself once the heir to the imperial throne of Austria, sees in Charles v "the image of an Emperor incarnate." He prefers the "golden age of medieval times" to the Renaissance; in Charles' vain struggle to reunify Christendom he sees "one of the decisive factors to explain the close relationship linking Charles v with the second half of our twentieth century." Drawing a parallel between the emperor's objectives and the aims of the Second Vatican Council, Dr. von Habsburg attributes to Charles v's policies an importance that "far exceeds their time and place," calling him "a guide towards the centuries to come."

In his personal, understandably nostalgic, interpretation of Charles v's place in history, the distinguished author fails to regret, or at least to consider, that the emperor's posture as defender of the faith implied his desire (as it had developed toward the end of his life) to obliterate Protestantism, that his religious intolerance kept the Inquisition busy in Spain

and elsewhere, and that he transmitted his intransigent attitude intact to his successors, seeding another century of wars under the banner of religion. In line with his unlikely portrait Dr. von Habsburg also presents Charles v as innocent of Machiavellian politics and as a man of "absolute moral rectitude." But the facts are otherwise. The emperor's correspondence with his ministers, for instance, reveals the extent of his power politics, often masked by hypocrisy. His sophisticated instructions were designed to "let George" do the ugly thing so long as the public would not learn that the orders came directly from the emperor. Even some of the older books listed in Dr. von Habsburg's meager bibliography (Giuseppe de Leva; Cardinal Granvelle's papers edited by Charles Weiss; Edward Armstrong) contain evidence that the emperor was no less a Machiavellian than his adversaries—he simply covered his tracks better. To give only a few examples: his perennial wars with France were as often provoked by him as by the French king; he had annexationist designs on parts of Italy not yet in his power; he secretly sanctioned various murders, including that of Pier Luigi Farnese (also an ancestor of Otto von Habsburg); he subtly instigated the War of Parma (which is not even mentioned by the author). This reverse side of the emperor's record may be explained, if not excused, as political expediency, but that is only another word for Machiavellianism. If anything links Charles v with our time it would be precisely his imperialism-cum-excellent-public-relations: the same policy of great powers assuming the right to decide by force, under brazen denials and pious slogans, the destinies of other peoples that is still with us. A far cry from the aims of modern ecumenism, the emperor's example, it would perhaps be truer to say, should rather warn than guide us.

Apart from this bias readers should find Dr. von Habsburg's book valuable as a testimony of a scholar's firm grasp of history from the unusual perspective of a born monarch. All in all, this is less a biography of Charles v than a narrative of the sixteenth century, concise and readable, that includes much interesting detail, especially on the German background.

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CARLO GINZBURG. *Il nicodemismo: Simulazione e dissimulazione religiosa nell'Europa del '500*. (Biblioteca di cultura storica, Number 107.) [Turin:] Giulio Einaudi Editore. 1970. Pp. xviii, 223. L. 3,500.

The term *Nicodemite* was coined by John Calvin in 1543, and he used it to describe those true believers who hid their faith behind a mask of outward conformity to Catholic rites. The term was raised to the rank of a historical concept by Delio Cantimori, to whom this book is fittingly dedicated; he, as well as his able follower, Antonio Rotondò, have used it to indicate those Italian heretics in the second half of the sixteenth century who adopted the practices condemned by Calvin rather than suffer the inevitable consequences of an open confession of their convictions. Thanks to a scholarly preparation that is as international in scope as were the religious movements he studies, Carlo Ginzburg has now shown Nicodemism to have been a doctrine as well as a practice, and he has discovered it fully stated almost two decades before it came to Calvin's attention. He has traced it back not to the Waldensians, who never tried to justify theoretically their age-old tactic of pretended submission, not to Juan de Valdés, who never consciously passed beyond the limits of orthodoxy, but to the ex-monk and humanist of Strasbourg, Otto Brunfels. Brunfels was indebted intellectually to Erasmus, Lefèvre d'Étaples, Zwingli, and Luther—or at least to Luther's *On Christian Liberty*. But it was he who, in reaction to the horrors of the Peasants' War, first picked out the pertinent Scriptural precedents: not Nicodemus' secret visit to Jesus, which was Calvin's discovery, but Namaan after his return to Syria and St. Paul on the circumcision of Timothy. And it was Brunfels who, in his *Almanach* of 1526 and in his *Pandects* of 1527, first worked out an ecclesiology from which Nicodemite practices could be deduced. The true church, said Brunfels, is purely spiritual. True Christians are made such by an act of faith, not by membership in any particular institution. Hence they can participate in even the most extravagant religious rites without harm to their souls. Indeed, with proper mental reservations they can even benefit from the kernel of truth still detectable beneath the superstitious superstructures of the Roman Mass.

The rapid spread of this doctrine in the 1530s is attested to by the frequent reprinting, translation, and plagiarism of Brunfels' many works, which the author has tracked down with amazing assiduity and commendable philological skill. The spread of Nicodemism can be explained largely by the expectations for a rapid solution to religious differences aroused by the Colloquy of Regensburg and the calling of the Council of Trent. Hence the timeliness of Calvin's diatribes: if too many French Huguenots followed the example of Gérard Roussel (whom the author identifies as the probable object of Calvin's wrath) in trying to work from inside the Catholic establishment, then the Word of God would soon be silenced. Hence also the reaction to the diatribes among the Italian heretics in 1550: an open letter from Wolfgang Capito (which the author here reproduces from its only extant version in the papers of the Bologna Inquisition) had just exposed them to Brunfels' doctrines. And it was these doctrines, along with Giorgio Siculo's adaptation of them to local conditions, that provided a justification for Nicodemite practices in Italy during the next half century.

ERIC COCHRANE

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ROGER D. MASTERS. *The Political Philosophy of Rousseau*. Princeton: Princeton University Press. 1968. Pp. xxiii, 464. \$12.50.

LESTER G. CROCKER. *Rousseau's Social Contract: An Interpretive Essay*. Cleveland: Press of Case Western Reserve University. 1968. Pp. xi, 198. \$6.95.

JUDITH N. SHKLAR. *Men and Citizens: A Study of Rousseau's Social Theory*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1969. Pp. viii, 246. \$8.50.

WILLIAM H. BLANCHARD. *Rousseau and the Spirit of Revolt: A Psychological Study*. Ann Arbor: University of Michigan Press. 1967. Pp. xiv, 300. \$8.50.

Jean-Jacques has hitherto been right: nobody understood him, despite his repeated pleas for understanding. Even worse, he claimed in his own time that he was misunderstood, often deliberately, by people who twisted his words, corrupted his meaning, and perversely refused to see the "truth" he so sincerely championed.

His nineteenth- and early twentieth-century critics carried on the practice. Rousseau would not admit that in the effort to verbalize his intuitive certainties he invited misunderstanding. In any case, as one sympathetic scholar has noted, all too many critics were ready to accept the invitation. Contemporary critics, however, have generally responded differently to Rousseau's maddeningly ambiguous pronouncements. What might be called the Morley-Taine interpretation—which held Rousseau's works to be frivolous, confused, irresponsible, and dangerous—has given way to a view that, while it includes severe criticism, is more sympathetic, more nuanced, and more seriously concerned with an objective analysis of Rousseau's brilliant and provocative contribution to social and political thought. Vaughan, Lanson, Wright, Cassirer, Schinz, Derathé, Starobinski, Burgelin, and Grimsley are among the initiators and continuators of this new critical canon. The books here under review are with one qualified exception all contributions to the new tradition.

Of the four, Roger D. Masters' study is the most ambitious. An "exegesis of the major works of political philosophy written by Rousseau," it is based on the assumption "that his philosophical writings form a coherent whole." Such an exegesis, Masters notes at the start, requires an exhaustive study of the sources and entails a finished work marked by "extensive quotations and . . . references which pepper the textual analysis." Thus forewarned, the reader is prepared to follow Masters through the intricate verbal maze of Rousseau's political thought. In just deference to his greatness, Masters accepts Rousseau's suggestion that the proper method of studying his works is to begin at the end, with the *Émile*: Rousseau, in his typically paradoxical way, insists that his first principles are unfolded only in his last writings. Again, Masters accepts Rousseau's view that the essential structure of his political thought is revealed in the *Émile* and the two *Discourses*. These three works, then, which Rousseau described as "inseparable," form the basic subject of Masters' analysis. He does not by any means neglect the *Social Contract*, but he maintains that only by following Rousseau's suggested course of study can we overcome the prevailing tendency to assume that that treatise

is riddled with contradictions and confusion. Retaining the assumption that Rousseau was a consistent, coherent political thinker, Masters' exegesis is thus as sympathetic as it is thorough. Masters strives conscientiously to determine precisely what Rousseau meant when he employed such terms as nature, natural law, law of nature, the general will, and freedom. He seeks to demonstrate that a careful attention to the spirit as well as the letter suffices to reduce greatly the confusion that surrounds Rousseau's work. Masters sticks tenaciously to his texts, eschewing biographical or psychological investigations, of which more below.

He ascribes much of the misunderstanding of Rousseau to the general failure of critics to note that Rousseau saw himself at once as a philosopher, and thus a moralist, and as a scientist, and thus a mechanist. Noting this combination of techniques and tendencies, Masters writes: "Rousseau thus found himself in the 'middle' so to speak, being opposed by two different schools of thought which were reflected among his contemporaries. To one side were those who denied any universal principles of human excellence; to the other, those who denied that physical necessity provides a satisfactory explanation for the origin of man's social life and moral life. Rousseau tried to resolve . . . these opposed positions and . . . he sought to reestablish the classical conception of the *polis* on the basis of modern physics; from the modern conceptions of man as a naturally apolitical (if not antisocial) animal, Rousseau tried to derive a regime which approximated, in certain ways, the political order discussed by those philosophers who assumed that man was a political animal." Accepting Masters' exegesis, we can more readily understand Rousseau's radical critique of society and his equally radical solution to political problems. But Masters' efforts to save Rousseau from inconsistency and incoherence at times require what may legitimately be termed overcharitable interpretations and remarkably flexible definitions. And even so, Rousseau's works remain replete with "tension," "paradox," and "apparent contradiction," terms that recur throughout the book. Masters' essay will be required reading for all serious students of Rousseau, but it will not persuade them all that the maverick philosopher was any more con-

sistent than the Enlightenment thinkers he disdained. In a valuable final chapter of "Critical Reflexions" Masters concludes that "Rousseau's philosophy must be accounted as a failure." The ultimate paradox of Rousseau is that the work he himself believed his weakest—the *First Discourse*—remains the most defensible and relevant of his writings, "the one which speaks most directly to the crises of our time." Masters, following Rousseau, holds that our modern predicament requires "a confrontation of the scientific optimism of modern opinion with the political pessimism of ancient philosophy," a sobering thought given the complexity of modern technological society and the inadequacy of what passes for political science, even when or perhaps especially when expounded by a Rousseau.

To turn from Masters' exegesis of Rousseau to Lester G. Crocker's interpretive essay on the *Social Contract* is to undergo a radical change of perspective and atmosphere. Masters is patient, sympathetic, indulgent; Crocker is severe, sarcastic, and judgmental. He emphasizes Rousseau's consistent reliance on duplicity, whether in educating Émile or in persuading the masses to enter into the social contract. He has no patience with Rousseau's terminological laxity, writing instead of "his usual arbitrary distortion of vocabulary." If he does not judge Rousseau but rather points only "to the implications and potentialities of what he wrote," there is no doubt that he finds Rousseau's political thought potentially pernicious: "I do not think that anyone could have read Locke, Jefferson, or Diderot and been inspired to construct a totalitarian system or State from their ideas." It is Crocker's clearly polemical approach to Rousseau that tempts me to exclude his work from the mainstream of recent Rousseau criticism. And yet Crocker is enormously informed on the matter; his analysis is rigorously disciplined, and his conclusions are most persuasive. His essay places the *Social Contract* within its appropriate historical and philosophical context and discusses Rousseau's program fully and fairly. In a very interesting chapter Crocker compares the implications and potentialities discernible in Rousseau's work to the gloomy realities depicted in the modern utopias, or anti-utopias, of Orwell, Huxley, and Skinner. His conclusion is a devastating critique

of all that Rousseau stood for. Although he grants that Rousseau was extraordinarily prescient in sensing the emerging dilemma of modern man living in a disenchanted world, he concludes that "his messianic dream of a millennial deliverance is a nightmare far worse than the reality he so properly condemned." Crocker's is an excellent essay, perhaps unfair at times but always incisive and provocative.

By comparison with Crocker's work, Judith Shklar's study of Rousseau's social theory is a restrained and relatively leisurely enterprise. Her interpretation of Rousseau is, however, both original and stimulating. She analyzes Rousseau's philosophical constructs as "models" that lead to critical understanding and judgment: the *Social Contract*, for example, "was not meant to be a plan for any future society, but a standard for judging existing institutions. It was a yardstick, not a program." This may well be, but Rousseau's contemporaries and posterity certainly saw it differently. Professor Shklar very ably demonstrates the impact on Rousseau of two earlier "models," Sparta and the "Age of Gold." She shows how Rousseau in the *First Discourse* used the Spartan model as the polar opposite to contemporary France, and she elaborates the lessons Rousseau drew from his Golden Age model, "the paradigm of the utopia of innocence." Sparta is the home of the man "denatured" into the perfect citizen, "virtuous" and without inner conflicts; in the Golden Age man is still natural, unspoiled, and "good." In subsequent chapters Mrs. Shklar analyzes Rousseau's "moral psychology," his emphasis on "the empire of opinion" (in which preoccupation Crocker discerns Rousseau the behaviorist-social conditioner), and his political and social thought proper. All of these chapters are solidly constructed and marked by a truly impressive grasp of Rousseau's elusive thought. At times Professor Shklar's formulations suffer from an excessive tentativeness as when she writes: "Rousseau may very possibly have overestimated the pain caused by inner and social conflict." That he did, tormented soul that he was, seems incontrovertible. At other times the reader finds it difficult to discern where Rousseau leaves off and Shklar begins, an understandable but disconcerting stylistic lapse. The excellence of this book is, however, undeniable. Professor Shklar's conclusion is

rather more pessimistic than Masters': "When he called upon his readers to choose between man and the citizen he was forcing them to face the moral realities of social life. They were asked, in fact, not to choose, but to recognize that the choice was impossible, and that they were not and would never become either men or citizens." This is a book that deserves the widest possible attention.

In a perceptive and intelligent postscript, "Considering Rousseau," Mrs. Shklar offers a valuable analysis of the methodological and conceptual problems relative to intellectual history. Among other questions she asks, "How important, for example, is biography, or to be exact, what sort of biography? In Rousseau's case the answer is clear. The private biography is of utmost significance." Lester Crocker is decidedly of this view, but Masters, for his part, concludes that Rousseau's biography is not "essential for an understanding of Rousseau as a philosopher." Without impugning the intrinsic worth of Masters' analysis of Rousseau's thought it seems impossible to deny that Professor Shklar's opinion carries greater conviction. Rousseau himself insisted that his thought and his personality were inseparable and had to be viewed as aspects of one entity. One of the salient facts about Rousseau is precisely that he was always examining himself—and publishing the results. He wrote three autobiographical works that are to a large extent conscious explications of what he felt he had said or meant to say in other writings. All of his political essays, all of his essays regardless of subject, are personal outpourings that far surpass in their self-revelatory quality Goethe's "fragments of a great confession." This aspect of Rousseau's writings is masterfully exploited by William H. Blanchard in his psychoanalytical exploration of the meaning of Rousseau and his political ideas. The relationship between history and the "ancillary sciences" is never more precarious than when psychoanalysis extends its offer of assistance. In my opinion, however, Blanchard's book should do much to strengthen the contacts between the two disciplines. Blanchard has done his research well; he knows Rousseau and his historical environment intimately; he is insightful in his discussion of the connections between Rousseau's personality and his politics, but he is never a

reductionist. This is a rich interpretive essay, persuasive I should hope even to the psychoanalytically uncommitted. Indeed, since it is libido- rather than ego-oriented in its approach it may draw more fire from the converts. That would be unfortunate. Space limitations preclude the generous treatment the work deserves. At the risk of oversimplifying Blanchard's delicate and nuanced assessment I will only point out that his central concern is "to trace the evolution of Rousseau's childhood sado-masochism to the moral foundation for his adult personality." Blanchard takes "this particular facet of his rich and varied psychological life as a basis for demonstrating the relationship between personality and political belief." He does this without recourse to psychoanalytical jargon, and his method indeed clarifies many of the obscurities and contradictions in Rousseau's work. True to his discipline, he does not label Rousseau "sick." He recognizes the inevitable tensions that afflict the human animal and out of which can come our greatest efforts: "There is no assurance that a well-adjusted Rousseau would have seen the problem faced by the hero of our story." Blanchard concludes that Rousseau the rebel had within him the attributes of the tyrant: "Rousseau understood the totalitarian mind because he partook of its very substance. . . . Rousseau's social behavior was primarily masochistic, but sadism and masochism are two aspects of the same drive, and Rousseau's political writing bears out the truth of this statement. Particularly in *Emile* . . . the urge to dominate, which he reacted against in his social behavior, became manifest both in the style and the substance of his writing." Rousseau's ambivalence toward freedom and authority, which all of his readers have noted, is rooted in the depths of his personality. We all share that ambivalence and for the same reasons. In his concluding chapters Blanchard, with Rousseau's example before him, offers some suggestive observations on the "morality of revolt," a contemporary problem that calls for renewed inquiry into unconscious motives and drives. It is, or should be, an inquiry whose function is not to preclude social change but rather to ensure that liberation movements do not degenerate into tyranny. Blanchard's sensitive essay and the other books here reviewed contribute much to our appreciation of Rousseau's life and work.

I think Rousseau would agree that we are today closer to understanding Jean-Jacques.

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O. F. SOLOV'EV. *Velikii Oktiabr' i ego protivniki: O roli soiuz Antanty s vnutrennei kontrrevoliutsiei v razviazvanii interventsii i grazhdanskoi voiny (oktiabr' 1917—iiul' 1918)* [The Great October Revolution and Its Enemies: On the Role of the Entente's Alliance with the Internal Counterrevolutionaries in the Unleashing of the Intervention and Civil War (October 1917–July 1918)]. Moscow: Izdatel'stvo "Mysl'" 1968. Pp. 326.

The theme of this book is accurately expressed in the title, although its specific significance is clarified by the subtitle, which explains that the study deals with the treaties that bound together the foreign interventionary powers and the internal counterrevolutionary forces and discusses the role this relationship played in bringing about the civil war and intervention.

The author begins his story with the formation of the entente in which the "bourgeois-landlord circles" of Russia helped to forge the treaty obligations that were one day to confront the Bolshevik government with its most formidable combination of internal and external opponents. The close relationship and basis for continued cooperation between these two hostile allies against the new regime were benefitted by the availability in the Allied capitals of the diplomatic staffs of the displaced Russian governments on the one hand and the easy accessibility of the interior of Russia to the Allied missions because of their long-standing acquaintances and friendships on the other. All these advantages, the author asserts, could be used by the Allies to achieve their central purpose—not, as they claimed, the marshaling of every effort to defeat the Germans but rather an objective they could not state so openly, the overthrow of the Bolsheviks and the Soviet government.

The book has in its favor the proportion of space and attention it gives to the year 1917 in the development of the intervention. Also in its favor is the book's general conception, which is to draw together materials from fields

of writing such as the intervention that have heretofore been separately treated and to focus them on a theme in a way that, with the observance of higher standards of scholarship, could be meaningful and worthwhile. But the way the author has done this leaves his product little more than a polemical exercise. The wide use of source material promised in the introduction fails for the most part to materialize, as few of these archival sources appear to have been used. Refutation or misinterpretation of many well-known works seems more satisfactory to the author's purpose. One can only hope that a serious study of this subject may someday be done by someone who will make meaningful use of the wealth of source materials that appears to be available for it.

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G. N. SEVOST'IANOV. *Diplomaticheskaiia istoriia voiny na Tikhom okeane: Ot Pirl-Kharbora do Kaira* [Diplomatic History of the War in the Pacific: From Pearl Harbor to Cairo]. (Akademiia Nauk SSSR, Institut Istorii.) Moscow: Izdatel'stvo "Nauka." 1969. Pp. 647.

Soviet works on international affairs are necessarily tendentious, and Sevost'ianov's book is no exception. The author's stated aims are both ambitious and admirable; his success in achieving them is another matter. He declares his main goal to be a comprehensive analysis of the policies of all of the great powers and some of the lesser ones during the period from the Japanese attack on Pearl Harbor to the Allied conferences at Cairo and Tehran. To make the war in the Far East comprehensible, the author reminds us, it must be placed in the general context of World War II, and it is particularly essential to try to judge the impact of the Soviet-German conflict on the struggle in the Pacific. No one would question the soundness of these necessities. But here the consensus ends.

Few Western scholars would concede that the war is best viewed as basically an imperialistic struggle for more colonies and enlarged spheres of influence. Nor would many who have studied the vagaries and inconsistencies of Anglo-American (not to mention Japanese and German) policy making in this period assert

quite so confidently that postwar considerations were crucial in determining politics and diplomacy during the first two years of the war.

Given the ideological constraints under which he works, most of Sevost'ianov's general conclusions are as predictable as they are untenable. For example, the United States sought to destroy Japanese power by force of arms, to undermine the foundations of the British Empire, and to create a kind of client state in Nationalist China, all for the purpose of making the influence of the United States supreme in the Far East and the world. In some essential details, however, this is a valuable book: Anglo-American divergencies in the Far East are cast in sharper relief and made more comprehensible by the inclusion of imperialistic factors in an enumeration of causes; Japan's cautious policy toward the USSR—particularly after the Red Army's victories at Stalingrad and Kursk—is convincingly demonstrated; so, too, are the recalcitrance and lack of political acumen of Chinese leaders, although Sevost'ianov is about as harsh with Chiang Kai-shek as was Joe Stilwell. Finally, the work is based on a wide range of published and unpublished sources and includes numerous quotations from otherwise inaccessible materials in Soviet archives.

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HUGH KEARNEY. *Scholars and Gentlemen: Universities and Society in Pre-industrial Britain, 1500-1700*. [Ithaca:] Cornell University Press. 1970. Pp. 214. \$6.75.

For a general critique of this book it seems wise to me to refer readers to Professor Lawrence Stone's review essay, "The Ninny-versity," *New York Review of Books*, Jan. 28, 1971, pp. 21-25. I shall use my space to state briefly Professor Kearney's thesis and raise some doubts about his support of it.

The medieval British universities had begun to adjust their curriculums to changing social needs about 1500. But the Reformation altered the direction of the flight from professional education. It became central to the government's purpose to control the universities, since it saw in the Renaissance cultural emphases of the universities a potential for

intellectual disaffection as well as a chief support of the powers that be. Hierarchy triumphed in the colleges after 1540, taking the special form of court humanism—the conservative ideology of Tudor loyalists, derived chiefly from classical models, and useful to men who fed at the public plate. Opposed to the elitist careerism of the gentry and their fondness for Plautus, Terence, Cicero, Aristotle, and Plato at various times stood the adherents of other muses: More's "civic humanists" in search of wider political involvement; Lawrence Humphrey's "country humanists" addicted to the Bible; numerous Ramists who opposed revived scholasticism with the bourgeois ideology of French dissent; and Baconians who wished a practical revolution in studies to defeat the abstract absolutism of Laud's reforms.

Professor Kearney's appetite for dichotomies is huge. But, as a recent essay of mine has demonstrated ("Humanists and Government in Early Tudor England," *Viator*, 1 [1970]), he is wrong on the typology of court, country, and civic humanists. He does not demonstrate the preference of humanists of Humphrey's kind for the Bible. Indeed, on page 102 Kearney reverses himself and makes of Humphrey Milton's model, an advocate of educational reform based on Pythagoras, Plato, Isocrates, and Aristotle! Puritanism, which in its urban wing adopted radical Ramist social criticism (p. 47), is elsewhere seen as the resolutely conservative stance of the gentry (p. 102). There is a constant confusion of traditional positive Biblical theology with a Ramist revolution (p. 50). Elitist court scholars in the 1630s were in fact base-born men thrust forward by Laud (p. 92). So cavalier is Professor Kearney with facts and categories that the Puritan peer Robert Greville, Lord Brooke is cited as evidence for the gentry's hostility to Laud's patronage of mushroom figures. It is confusing to find country types who are elitists in opposition to leveling court patrons, but no more so than to find the universities in 1629 still "republican" in character when we have already read about the victory of the central government over them in the early stages of the Reformation (pp. 19–37, 93–94).

This book may be bold in conception; it is bad in execution. This applies as well to tables with no obvious reference in the text,

an index that is unreliable, and the rules of interpreting evidence. Professor Kearney sacrifices sound methods to his larger view, yet another schematic effort to provide radical social and intellectual parents for the English Revolution. The attempt is unconvincing.

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LESLIE GARDINER. *The British Admiralty*. Annapolis: United States Naval Institute. 1968. Pp. 418. \$8.00.

The title of this book conjures up an image of a vast centralized organization dispatching gunboats up the Yangtze, controlling great fleets on the high seas, and sending messages to Malta, the Cape, and Singapore. The term "British Admiralty" seems to evoke a body with a unity and magisterial direction that was often far from the reality of the situation. In the mid-eighteenth century, for example, when the navy covered half the earth, it was managed by ten elderly principal officers and thirty-eight clerks.

Mr. Gardiner's book is the first comprehensive study of the administration of the Royal Navy. Earlier works were often cumbersome, parochial, or both. The author has not consulted the Admiralty Papers at the Public Record Office and has furnished only a scant bibliography. Yet to research thoroughly such an unwieldy subject would certainly be the work of several lifetimes. What Mr. Gardiner has accomplished is the difficult task of writing a clear and readable account of almost five hundred years of naval administration. He leads us from the awesome and unseaworthy ships of Henry VIII's time—with their gilding and statuary, requiring "a College of Heralds, not an Admiralty . . . to look after them"—to the gray dreadnoughts of the twentieth century. He traces the innovators and time-servers who managed the navy; it is often a sorry record of chicanery and the triumph of patronage over ability.

Before Fisher's reforms in this century the Admiralty frequently lagged in essential development: in Nelson's time the French and Spanish often had better ships, and in the nineteenth century the French built the first

seagoing ironclads. But we have to thank the Admiralty for many quiet achievements, such as its superb charts, the unsung anti-slavery patrols—often decimated by disease—and for its historic voyages like Darwin's in *H.M.S. Beagle*. These and many other activities were undertaken with very little cost to the taxpayer.

The seventeen years that Mr. Gardiner spent in the Royal Navy have undoubtedly given him an appreciation of Britain's long record of naval administration. This book is a lively account of what could have been a rather prosaic subject.

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HORTON DAVIES. *Worship and Theology in England. Volume 1, From Cranmer to Hooker, 1534-1603*. Princeton: Princeton University Press. 1970. Pp. xix, 482. \$15.00.

JOHN S. COOLIDGE. *The Pauline Renaissance in England: Puritanism and the Bible*. New York: Oxford University Press. 1970. Pp. xiv, 162. \$6.75.

Professor Horton Davies began his five-volume survey of English religious life since the Reformation with volume 3. The present volume 1 is actually the fourth volume to be published. In pushing the story back to the fashioning of the Church of England under Thomas Cranmer and in detailing the struggles that ensued among Anglicans, Puritans, and Catholics, Professor Davies again exhibits his disciplined scholarship, wide acquaintance with the sources, and graceful and lucid literary style. Since the issue of Biblical authority was central to the debates of the period, it is unfortunate that the volume was written prior to the publication of John S. Coolidge's *The Pauline Renaissance in England: Puritanism and the Bible*.

Coolidge's book appears without fanfare. The jacket gives no hint that it is a study of first-rank importance, clarifying in a remarkably perceptive way an issue that John Whitgift and Richard Hooker in the sixteenth century were never able fully to understand and that has continued to puzzle and confuse subsequent scholars who have sought to make sense of the Puritan critique of Elizabethan conformity.

Coolidge makes it clear that both parties grounded their arguments on the authority of Scripture. Moreover, they were in agreement on principles of scriptural interpretation and application. The Puritans, with Thomas Cartwright as a chief illustration, were not Biblical legalists who sought precise and specific directions (proof-texts) in the Bible. They acknowledged that practices must vary by reason of "times, places, persons, and other circumstances." Cartwright, like Whitgift and Hooker, operated on the basis of general precepts that were variable in application according to circumstances, and he was equally insistent that one could not be tied to a precise application of a specific text.

A subtle distinction was at the heart of the controversy. While Whitgift and later Hooker argued that indifferent matters are to be ordered in a manner "not repugnant to" the general precepts of Scripture, Cartwright maintained that they must be ordered "according to" the general precepts of Scripture. When Whitgift pointed out that there was no logical or rational difference between the double negative and the positive statement, Cartwright agreed. Though Cartwright could not deny the logical sufficiency of the double negative, still he found it wanting. It was not logically or rationally inadequate, it was religiously inadequate.

The double negative suggested to the Puritans an indirect and incidental kind of agreement with Scripture, resulting in a passive rather than an active obedience. The Puritans were preoccupied with nuances of meaning, with a sense of obligation that was conveyed by the positive statement but not by the double negative. This was the point Whitgift and Hooker were unable to grasp, the point that has been missed by subsequent interpreters. Though it may seem a quibble, Coolidge notes that it was rooted in a renaissance of Pauline thought that was especially pertinent in a time of cultural dislocation. Two understandings of the meaning of obedience were involved—"the scriptural sense" of obedience as "response to God's word" and "the rational sense" of obedience as "conformity to God's truth."

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ALAN MACFARLANE. *Witchcraft in Tudor and Stuart England: A Regional and Comparative Study*. (Torchbook Library Edition.) New York: Harper and Row. 1970. Pp. xxi, 334. \$8.50.

Sixty-two years ago Wallace Notestein received the Herbert Baxter Adams Prize for his pioneering study of witchcraft in England during the Tudor and Stuart periods. His work did much to dispel misinformation and erroneous views, but with the passage of time its inadequacies in research methods and in approach have become increasingly apparent. Notestein worked mainly from contemporary pamphlets, only sampling records of witchcraft trials; modern scholarship demands exhaustive study of all surviving documents.

In order to make such research manageable Macfarlane has limited himself to the county of Essex, which he chose because of the excellence of its archives and because an unusually large number of witches were tried there. Most prosecutions in the assizes had already been listed by C. L. Ewen; Macfarlane has re-examined all this material and corrected a few minor errors. More important, he has searched the records of Essex quarter sessions, of the ecclesiastical courts in the archdeaconries of Essex and Colchester, and of some Essex boroughs. These, together with a few miscellaneous sources, enable him to provide an appendix enumerating more than twelve hundred cases of witchcraft.

Many significant points emerge. Prosecutions were scarce before 1542 and began in earnest after the passage of a new witchcraft statute in 1563. Peak years for indictments in Essex were 1584 and 1645. After 1680 the passion was largely spent. Those accused of witchcraft were generally older women; their victims were nearly always younger, although seldom children, and were of both sexes. In most cases accused and accuser lived in close proximity, usually in the same village. Those accused were poor but not the poorest in the community; thus economic factors do not account directly for prosecutions. There was little correlation between witchcraft and other crimes or sexual offenses. The amount of illness and death attributed to witches was very small in comparison to known mortality. Witches were not blamed for deaths due to plague. There was

little connection between Puritanism and witch-hunting. English witches, unlike their continental counterparts, were not accused of compacts with the Devil and did not celebrate witches' sabbats.

The weakest portion of Macfarlane's book is its concluding section, in which he compares his conclusions to those of anthropologists who have studied witchcraft among modern African tribes and American Indian groups. Interesting as some of this material is for its own sake, it sheds little light on the situation in England, where conditions and beliefs were quite different.

Professor E. E. Evans-Pritchard has contributed a preface in which he suggests that Macfarlane has asked the right questions because of his readings in anthropology. That may be. But I think it more likely that Macfarlane has done the right thing because he is an unusually able practitioner of the craft of local history.

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CHARLES WILSON. *Queen Elizabeth and the Revolt of the Netherlands*. Berkeley and Los Angeles: University of California Press. 1970. Pp. xiv, 168. \$6.95.

The relations of England and Holland through the centuries have been Charles Wilson's business for many decades, to the common profit of his homeland and the land across the narrow sea to the east. Hitherto his interest has been chiefly in economic history, but now he caps his long career with an important little book in diplomatic history, originally presented as the Ford Lectures at Oxford in 1969. An economic historian but no determinist, a sober scholar of more than a few years who demolishes established reputations with almost boyish enthusiasm, Wilson scrutinizes the policies followed by Elizabeth I toward the Netherlands in revolt against Philip II. He rejects the traditional picture of the prudent Protestant queen, slyly aiding William the Silent and the Dutch rebels while averting as long as she could the direct collision with Spanish power. He paints instead a querulous monarch who avoided decisions when decisive action might have salvaged the unity of the Low Countries as a single independent country

and then leaped to sudden resolutions at the wrong time and with unwanted results. She detested the Netherlanders, both the overmighty nobles and the grubby merchants, who rose up against the Spanish monarch from whom she had so much to fear, and she aided them, reluctantly and parsimoniously, only because she feared France, then crippled by civil wars, more than Spain at its height of power. An incompetent Leicester is no novelty in the historical literature but a Burleigh who could have been the model for Polonius is. Yet Wilson is not interested in reputation-smashing as such; his portrait of William of Orange is admiring and warm, that of Parma admiring and cool. There are many small treasures in this short book. Not least is Wilson's re-examination of Pieter Geyl's thesis that the political division of north and south resulted from the immense importance of the great rivers as military barriers. Wilson demolishes their significance with a command of military history such as Geyl did not possess. Yet I think that he shows, in a way different from Geyl's, that the course of battle—that medley of the aleatory and the necessary—played at least as great a part in the division of the Netherlands as the different social structures of Holland and Hainaut, which Wilson prefers to stress. Lectures are by nature terse, more assertions of a position than their proof. It is to be hoped that Wilson's dicta will lead more than one historian to more detailed studies of these significant events, and prove him right or wrong.

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VERNON F. SNOW. *Essex the Rebel: The Life of Robert Devereux, the Third Earl of Essex, 1591-1646*. Lincoln: University of Nebraska Press. 1970. Pp. xv. 515. \$15.00.

This is the first full-length biography of a man who in his own day was immensely prominent. He was the son of Elizabeth's glamorous and unfortunate favorite, and he never forgot it. In his early twenties he was a central figure in the most celebrated divorce case of the seventeenth century. In Charles I's early Parliaments he was among the leaders of the opposi-

tion in the Lords. From the beginning of the Long Parliament he collaborated with Pym; he was Parliament's "Lord General" until his disaster in Cornwall in 1644 and the subsequent Self-Denying Ordinance, which he bitterly opposed. When he died he was given the most elaborate funeral England had seen since that of James I. Fame is fleeting; he has been almost entirely neglected by posterity.

Professor Snow's biography, an overlong narrative that suffers from too little analysis of events or of Essex' purposes, indicates that this neglect is not unfounded. Try as he will, Snow cannot breathe life or significance into the career of an unintelligent and narrow-minded man whose prominence was owing entirely to accident of birth, and in his last pages the author virtually admits as much. "Rebel" is really too grandiose a term to use for Essex; "malcontent" would be better. He was scandalously put upon during the famous divorce case, but his disastrous second marriage suggests that there may be more to be said for Frances Howard than is usually supposed—Essex was known as "his Oxcellency" to the Royalist wits of the 1640s. Essex was irritated at being passed over for high military command in the 1620s, though those who received preferment were far more experienced than he. He felt snubbed again because the king would not trust him during the Bishops' Wars. No doubt Charles might have been more conciliatory, but nothing during Essex' period of command from 1642 to 1644 suggests that Charles' military judgment was very wrong.

As one might expect from Snow's previous work he is at his best in discussing Essex' activities in Parliament. His description of Essex' logistical problems in the Civil War is also very interesting, though he sticks so closely to Essex' campaigns that the overall picture of the war is distorted, and the maps in this book resemble something out of Stephen Potter. The author's style is pedestrian; he occasionally misuses words; and there are far too many careless slips—Louis XIII was not Charles I's father-in-law (p. 182); Alexander III was not the pope who denied Henry VIII his annulment (p. 66); there is a vital "but" omitted from a quotation that makes nonsense of Snow's account of the trial of Fiennes for the surrender of Bristol

(p. 406). This is, in brief, an only occasionally rewarding narrative of the life of an uninteresting man.

MAURICE LEE, JR.
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MICHAEL J. BROWN. *Itinerant Ambassador: The Life of Sir Thomas Roe*. Lexington: University Press of Kentucky. 1970. Pp. xv, 302. \$9.95.

In Sir Thomas Roe Dr. Michael Brown has found an excellent biographical subject. Early in the reign of James I, Roe began his career by leading an expedition to South America in search of gold. Later, as a diplomat, he served the English government in wide-ranging areas of the world: India, Turkey, Germany, and Denmark. Dr. Brown recounts fully and clearly the various negotiations undertaken by Roe. He describes the evasions, delays, and trickery that the ambassador encountered at the court of the Great Mogul in trying to strengthen the commercial position of the East India Company. He shows how strife in the Turkish state hindered Roe's diplomatic efforts and how Mohammedan violence placed him and other Christians at Constantinople in frequent physical danger. Yet through courage, dexterity, and persistence he was able to achieve most of his aims. The author stresses particularly the effect of Roe's forceful and dignified bearing in raising the reputation of England in India and the Ottoman Empire.

Throughout life, as Dr. Brown points out, Roe remained an Elizabethan living out of his time, and it was this that invested his career with a sense of failure. An ambassador in Germany and Denmark during the Thirty Years' War, he worked for the creation of a Protestant alliance supported by England and for the restoration of Elizabeth of Bohemia to the Palatinate. With one notable exception—Swedish intervention in the Thirty Years' War—Roe failed to gain his major objectives. Because of the conflict of Charles I with Parliament and the consequent shortage of money, the king lacked both the means and the will to support Roe's diplomatic endeavors.

Dr. Brown has provided a competent and well-balanced portrait of a man who combined personal charm and integrity with professional skill of a high order. He clearly admires Roe,

but he does not neglect to point out some of his human frailties: a tendency to complain about physical discomforts and a readiness, almost universal at the time, to employ flattery for the purpose of self-advancement. On occasion the author's style is rather pedestrian. The use of many short quotations from Roe's correspondence, moreover, impedes the flow of the narrative. Dr. Brown might have reduced the number of these and given longer passages that would have more fully brought out the flavor of his subject's personality.

AMOS C. MILLER
University of Houston

G. DYFNALLT OWEN, edited, with an introduction, by. *Calendar of the Manuscripts of the Most Honourable the Marquess of Salisbury, K.G., P.C., G.C.V.O., C.B., T.D., Preserved at Hatfield House, Hertfordshire. Part 21 (1609-1612)*. (Historical Manuscripts Commission, Number 9.) London: H.M. Stationery Office; distrib. by British Information Services, New York. 1970. Pp. xxiii, 512. \$40.50 postpaid.

This calendar of manuscripts, the final volume from the collection of Robert Cecil, earl of Salisbury, contributes little, as its editor observes, to the understanding of major events, domestic or international. The relevance to scholars, however, of any of its 375 pages can be quickly determined by the thoroughness and accuracy of an index extending to 135 pages. The documents do reveal, for 1609 to May 1612, the multifarious aspects of administrative routine—concerning, for example, public revenues, trade, piracy, forests, requests for recompense or office, and pleas for mercy. Moreover, a large proportion of the more than 600 letters addressed to the earl of Salisbury, constituting some two-thirds of the total number of items calendared, came from the king and important officials at home and abroad. Two excerpts from letters of James I to his lord high treasurer will here be singled out. The king, whose passion for hunting made "my little beagle" an appropriate way to address the earl of Salisbury, wrote: "I shall never desire to be thought a great king if the reputation of an honest man be not joined thereunto" (p. 173). "I never use to change my affection from any man except the cause be printed on his forehead" (p. 265).

The editor has distinctly enhanced the value

of this publication by his introduction of nineteen pages. Here he describes in some detail four "notable events" of which the subsequent documents afford only glimpses; but he has drawn extensively on State Papers and other sources for his full analysis of the topics. The first has to do with drafting the Twelve Years' Truce between Spain and the Dutch United Provinces and with England's role therein. The second deals with the dissemination abroad of King James's aggressive defense of his views in a new edition of his *Apologie for the Oath of Allegiance*, the oath then being required of English Catholics. The third episode was the deep anxiety in London and English diplomatic circles abroad over the welfare of King James because of the assassination of the French King Henry IV on May 14, 1610. The feeling of alarm both encouraged protective measures and gave profitable opportunities to "unsavoury characters." Fourthly, there was a crisis in the king's finances and the abortive negotiations with Parliament for the "Great Contract." The crisis was temporarily eased by the sale of Crown lands, but was not surmounted by other measures that Salisbury took before his death on May 24, 1612.

WILLSON H. COATES
University of Rochester

ALAN MACFARLANE. *The Family Life of Ralph Josselin, a Seventeenth-Century Clergyman: An Essay in Historical Anthropology*. New York: Cambridge University Press. 1970. Pp. xiii, 241. \$9.50.

The diary of Ralph Josselin, who became vicar of Earl's Colne in Essex in 1641 and contrived to keep his post through all the changes of regime until his death in 1683, was published—greatly abridged—in the Camden Series in 1908 and has been since then a useful minor source of examples and quotations. Mr. Macfarlane has now made it the basis of an "essay in historical anthropology." He has analyzed Josselin's economic activities as a "yeoman-priest," his attitudes as a son, husband, and father, his relationships with his remoter kin and with friends and neighbors, and even his recorded dreams. In drawing comparative conclusions about seventeenth-century English society Macfarlane reiterates constantly that evidence from one such source is "obviously

inadequate" and that only much wider research of the same kind could establish the generalizations. The book must, therefore, be seen as an experiment and, perhaps, as a plea to historians to be less mistrustful of the methods of social anthropologists. But such caution will not altogether dispel the suspicion that more weight is being put on the diary than it can reasonably bear. "The first sentence of the diary shows that children were eagerly welcomed by their parents." Or does it merely show that one moderately puritan clergyman naturally ascribed to his own parents the acceptable attitude to the birth of a son? Deductions from fact rather than comment are more convincing, though even here diagrams and percentage tables analyzing contacts with such characters as a "fabro" (father's brother) may produce a mixture of fascination and skepticism. Nor does Mr. Macfarlane's arithmetic help his cause. The economic calculations accept too readily that any figures are better than none, and several references to the ages of Josselin and his relations seem incompatible with their dates of birth.

Nevertheless, historians, whatever their old-fashioned quibbles, will find the book valuable as well as enormously enjoyable. The section on "the mental world" is especially successful. Here for once is an approach to the history of ideas that gets below the level of published philosophy and preaching to the muddles, anxieties, and assumptions of one very ordinary man.

D. H. PENNINGTON
Balliol College

CHRISTOPHER HILL. *God's Englishman: Oliver Cromwell and the English Revolution*. (Cross-currents in World History.) New York: Dial Press. 1970. Pp. 324. \$7.95.

One always looks forward to the publication of a new book by Christopher Hill, for there is no one writing today who can rival his knowledge of seventeenth-century England and few who can present material with his grace of style. Yet it must also be said that *God's Englishman* and *The Intellectual Origins of the English Revolution* (London, 1965) have not lived up to their promise. It must be stated at once that *God's Englishman* is not a complete biography of Oliver Cromwell and that

the biography by Sir Charles Firth still stands supreme in the field. It is also clear that Mr. Hill never intended to supersede Firth.

The title of the book was very cleverly chosen. Cromwell thought of himself as God's good servant, and Hill portrays him and his relationship to the English Revolution almost solely in terms of the Puritan religious impulse that moved both Cromwell and the English revolutionaries of the period. Hill has for so long been considered a leader in the school of historians who consider history to be the unfolding of some great inexorable plan of social and economic progress that it is strange to find him here inclining almost to an acceptance of the great-man theory. Yet he stops short and is content to show that the life of Cromwell and the history of the English Revolution fuse into one entity. Neither was fully the master of the other.

In his opening survey of post-Elizabethan England, Hill tells us that seventeenth-century England and the Continent were both struggling to free themselves from the feudal past. The result was a series of revolts and civil wars everywhere in the attempt to install capitalism and the social and political institutions that must accompany it. Having said this, Hill proceeds to recount the history of the early Stuarts with an emphasis not far from that which one would find in S. R. Gardiner. Nothing novel here.

Oliver Cromwell is introduced as a typical Puritan country gentleman, deeply interested in the welfare of his community, Huntingdonshire and the Fens. With the assembling of the Long Parliament, he quickly emerged as a political and military figure of consequence. But it was the Civil War that brought out the true strengths in his character. It was Cromwell the soldier who became the great political leader. Behind the commander of the New Model Army and the lord protector of the 1650s was the man of God. It is here that we have the burden of the book. Cromwell and England had become one, and the force that united them was Providence. It was not the traditional Calvinism as a philosophical system but the Calvinism of predestination. Oliver Cromwell was the elected of God. It was this faith that moved him and in turn moved those

with whom he worked. The grace once gained could never be lost.

The book is disappointing since it is not the full-scale life that had been hoped for. It is, however, a fascinating study into the impact that predestinarian Calvinism had on the central figure of seventeenth-century England. Its final fascination is in the picture we get of Christopher Hill himself. The English Revolution was indeed a Puritan Revolution.

STUART E. PRALL
Queens College,
City University of New York

MALCOLM I. THOMIS. *The Luddites: Machine-Breaking in Regency England*. (Library of Textile History.) [Hamden, Conn.:] Archon Books. 1970. Pp. 196. \$7.50.

J. T. WARD, editor. *Popular Movements c. 1830-1850*. (Problems in Focus Series.) [New York:] St. Martin's Press. 1970. Pp. ix, 206. \$8.00.

D. J. ROWE, editor. *London Radicalism, 1830-1843: A Selection from the Papers of Francis Place*. (London Record Society Publications, Volume 5.) [London:] the Society. 1970. Pp. xxviii, 266. £3.75.

"Can you keep a secret?" asked a Luddite who had been mortally wounded in an unsuccessful attack on a mill. "Yes, yes, I can," replied the Anglican clergyman attending him, eager for the information that might come in a dying confession. "So can I," replied the Luddite, and he and his comrades kept their secrets so well that historians have had difficulty penetrating the movement. There is general agreement on placing Luddism in the context of economic distress and the introduction of new machines and practices. But the potential sources for viewing the Luddites in broader perspectives are more difficult to use: oral traditions transcribed many years after the events, reports of spies, confessions of captured Luddites, and letters from local magistrates. E. P. Thompson employed these sources in his important discussion of the movement in *The Making of the English Working Class*; he used them to examine machine breaking in the context of the attitudes, values, and other activities of the working-class community.

Now Thomis has reassessed the Luddites and has challenged much of Thompson's in-

terpretation by taking Luddism out of the context of the working-class community and limiting it to an industrial and economic framework. Of the two, E. P. Thompson's account is clearly the more satisfactory. Thomis is strangely insensitive to the possibility that working-class values may not have reflected economic "reality." And his interpretation hangs on proving the uselessness of the wide range of sources E. P. Thompson employed; but Thomis dismisses these sources too abruptly and too sweepingly to be truly convincing.

In sharp contrast to Luddism there is an enormous amount of material about the agitations and movements of the 1830s and 1840s. The sources are rich and varied; the historian's task is to deal meaningfully with the bulk and variety. To do this historians have recently shifted from attempting narrative chronicles on a national scale to more analytic local studies. Too frequently the result has been to leave students with old accounts that do not reflect the changes in questions and interpretations. *Popular Movements* is an attempt to close this gap by introductory essays of about twenty pages on parliamentary reform, the factory movement, anti-poor law agitation, trade unionism, Chartism, anti-corn law agitation, Irish repeal agitation, and the public health "movement." The contributions have been gathered from good historians, and the essays are interesting and useful. In most cases the emphasis is on the basic chronology of the movement. But questions of interpretation are central to some of the contributions and are not ignored by the others; each essay has a very helpful annotated bibliography.

The emphasis on the vigor and extent of provincial activity in *Popular Movements* is partially the result of the lack of published work on London radicalism during the period. Dr. Rowe has taken a step toward filling this gap with a general survey in *London Radicalism, 1830-1843*. The book is composed of selections taken from the collection of Francis Place, who, unlike the Luddites, revealed everything he knew in hundreds of volumes of letters, pamphlets, newspaper clippings, and unpublished writings. The most informative selections include correspondence and details relating

to movements and events in which Place was directly involved, particularly the National Political Union, and the Metropolitan Parliamentary Reform Association of the 1840s. There is also material about associations with which Place had no connection such as the National Union of the Working Classes and the Chartist Convention of 1839. The extracts are introduced by a discussion of Place's life, London radicalism, some of the individual associations, and some of the difficulties with using Place's material.

The Place Collection is a good source of information for events in which Place was directly involved, but it must be used critically. He magnified the importance of these events and of his role in them. His material is of much less use in dealing with working-class radicalism, with which his acquaintance was secondhand in the 1830s; he neither understood nor liked it. His judgments of its leaders were very harsh: he emphasized their defects, questioned their honesty and sincerity, and ignored their merits. The book is an accurate reflection of the Collection in that it documents a great deal more than it explains. It would have been a help in overcoming this problem if Dr. Rowe had annotated the extracts. And the book would have been much easier to use if the material had been divided into sections, given titles, and listed in a table of contents. As it is, the reader is left to find his way as best he can, still awaiting the publication of major studies on London radicalism.

THOMAS MILTON KEMNITZ

University of New Hampshire

F. C. MATHER. *After the Canal Duke: A Study of the Industrial Estates Administered by the Trustees of the Third Duke of Bridgewater in the Age of Railway Building, 1825-1872*. New York: Oxford University Press, 1970. Pp. xx, 392. \$13.00.

This immensely detailed work on the Lancashire properties left by the third duke of Bridgewater provides the first book-length account of the economic history of an English landed estate in the nineteenth century. Anyone who fails to note the book's subtitle, however, and expects to learn about agricultural

rents and farm management should be warned that Mr. Mather has nothing to say about such matters. For the Bridgewater estate was largely industrial, comprising canals, docks, warehouses, boatbuilding works, and collieries. It occupied a central position in the rising industrial-urban complex of southeast Lancashire, and therefore its history has much to tell the reader about transport and mining developments in that vital area and about the persistence and strength of aristocratic influence in the non-agricultural sector of English economic life.

The principal theme of the book is the survival of the Bridgewater enterprises, especially the Duke's Canal (familiarily known as the Duke's) into the later years of the nineteenth century, despite the increasing competition of the railways. Indeed survival is too weak a word. According to the author, "the early 1840's were once considered to have been the crucial years for the overthrow of the canals by the railways" (p. 121), but as he goes on to demonstrate, the mid-1840s were boom years in the history of the Duke's Canal. In 1844, for example, it showed a net annual profit of £76,410, and its total volume of traffic had grown from 716,000 tons in 1830 to 1,280,000 tons. Even as late as 1860 it would appear that the canal was enjoying an Indian summer, having earned a net annual profit of something like £58,500. In 1872, together with the Mersey and Irwell Navigation, the canal was sold for the not inconsiderable sum of £1,115,000.

Other and lesser themes in the book deal with Bridgewater estate agents, among whom James Loch was chief, and with the Worsley collieries, which are given a separate chapter. Loch's administrative policy is carefully assessed, and the management of the Worsley mines is helpfully placed in the context of nineteenth-century coal mining. A complaint might be ventured about the shadowy existence some of the principal characters lead, especially the aristocratic ones, obscured as they are by the flood of information about freight rates and railway-canal negotiations. But this is a small matter in a work that is at once useful and admirably patient and lucid in its execution.

DAVID SPRING

Johns Hopkins University

TIM PAT COOGAN. *The I.R.A.* New York: Praeger Publishers. 1970. Pp. x, 373. \$8.95.

Tim Pat Coogan, whose previous book was also a pioneer effort in modern Irish history, has provided the first comprehensive study of the Irish Republican Army from its inception to the present day. Perhaps the general reader would have been better served if Coogan had devoted more attention to the IRA during the period of the Anglo-Irish War and the subsequent Civil War, that tempestuous era that so decisively shaped the composition and character of the organization. There are a number of excellent histories that do, however, and Coogan's significant contribution lies in his depiction of the IRA over the past four decades. A surprising amount of documentary evidence seems to have survived, owing to the IRA's curious penchant for keeping voluminous records that all too frequently fell into the hands of both British and Irish authorities. The author was, moreover, invaluablely assisted in his research by the inimitable access he had to hundreds of people who either were members of the IRA or were tangentially associated with it. Accounts of the bombing campaign in England, of the relentless guerrilla activities in the North, and of the years of internment in the Curragh contain vivid characterizations of events heretofore shrouded by rumor and misinformation.

Irish-American efforts to assist the IRA were not hugely successful, and the author correctly observes how in the post-treaty years many Irish-Americans preferred smuggling sweepstake tickets in and out of Ireland to the less profitable and more hazardous task of supporting revolution. But Coogan is on less solid ground in contending that de Valera's American tour was so successful that by Christmas of 1920 the British believed United States funds and public opinion could support the Irish rebellion indefinitely. Recently opened British public records on this period reveal that the London government reached a decidedly different conclusion. The IRA received still less assistance from Stalinist Russia, and later Nazi Germany, as both powers regarded the organization as inept. Indeed the IRA seemed at times potentially more hazardous to itself than to its professed enemy, Great Britain.

There are countless stories of misfired plans, ill-timed attacks, and self-inflicted casualties that would make for humorous reading were not the consequences so tragic. Coogan's own abhorrence of the violence of the IRA does not prevent him from recognizing the integrity and courage of many of its members. The author's own courage is manifest in the concluding chapter, where he credits the civil rights movement of 1969 as having done more in one year to end injustice than the anti-partition forces had in fifty years, a bold statement for a Dublin journalist in a day when terrorist activity is again on the ascendancy in Ireland.

THOMAS E. HACHEY
Marquette University

GEORGE SAYERS BAIN. *The Growth of White-Collar Unionism*. New York: Oxford University Press. 1970. Pp. xvi, 233. \$9.00.

Though Bain intends this book to be a contribution to the considerable body of theoretical literature on union growth, his major purpose is emphatically practical and is related directly to the present position and future prospects of the British trade-union movement. Since the number of manual workers, the traditional base of British unionism, is declining while the number of the white collared is growing so rapidly that they will, by the 1980s, constitute the majority of the labor force, it follows that the union movement will be able to "maintain its relative position in the power structure of this country and . . . continue to play an effective role in the industrial relations system" only if it is able to recruit white-collar workers. "If it does not or cannot, the best it will achieve is numerical stability within an increasingly narrow band of occupational distribution, and its ability to advance even the interests of its manual membership will be seriously impaired" (p. 1).

Restricted to manufacturing industries and to the study of growth in terms of membership, the book focuses primarily on an analysis of the variations in the industrial and occupational pattern of white-collar unionism as it existed in 1964. The choice of that year was dictated by the evidence available: at no earlier point was it found sufficiently detailed

to permit the construction of a nationwide pattern of membership distribution. It is Bain's assumption that a systematic study of the variations in the density of white-collar union membership in the mid-1960s will enable him to identify the factors that promoted growth in the immediate past, and he assumes that these will continue to operate in the future. His procedure is to take from the literature of the subject—polemical, promotional, and scientific—and from his own interviews and questionnaires a long list of variables to which causal significance has been attributed and to test each of these against the evidence. The result is some firm conclusions: most of the variables are eliminated as having no general or significant effect, and three are singled out as controlling considerations. "The model claims that the growth of aggregate white-collar unionism in Britain can be adequately explained by three strategic variables—employment concentration, union recognition [by employers], and government action [to encourage recognition]" (p. 187). These conclusions challenge ideas commonly held, notably the assumption that economic benefits are the principal attraction of unions for white-collar workers. Rather it is shown here that they join primarily for the purpose of gaining control over the work situation, especially the rules that govern them on the job. "Given that employment concentration and bureaucratization will continue, trade unions will be just as necessary and useful to the white-collar workers of the 20th century as they were to the 'sweated' manual workers of the 19th century" (p. 188).

Historians will feel that, in dealing with such a concept as growth, a more strictly historical approach would have been in order. Taking the study on Bain's own terms, they are likely to complain that insufficient attention has been given to the interaction of the many variables and that studying them one at a time is bound to lead to oversimplification. It will be noted, moreover, that there is no attempt at testing the positive causal connections discerned in the analysis in terms of the growth histories of particular unions or groups of white-collar workers. Nonetheless, Bain's performance throughout this book is impressive. The depth of his research and his deployment of it, the thoroughness of his argument, the caution of

his estimates and conclusions, and his application of social science methods of analysis all make his study an instructive one for historians, especially for those concerned with the "new history" of the 1970s.

H. W. MCCREADY
McMaster University

JOAN EVANS. *Monastic Iconography in France: From the Renaissance to the Revolution*. New York: Cambridge University Press. 1970. Pp. xv, 76, 116 plates. \$19.50.

Joan Evans' slender new book is essentially an appendix to her earlier work, *Monastic Architecture in France* (1964) and can be used most conveniently and successfully in conjunction with the previous study.

The formats are similar: a brief and simplified introduction to the history of the period followed by eight short chapters devoted to the major monastic orders. The Benedictines absorb most of Miss Evans' attention (fourteen pages), a reflection of the more extensive development of the order in France as well as a reminder of the author's lifelong interest in the mother house and her classic study of its art (*Romanesque Art of the Order of Cluny* [1938]). The Cistercians, Augustinians, Carthusians, Carmelites, Dominicans, Franciscans, and Jesuits are awarded between two (Dominicans) and ten (Augustinians) pages apiece; still briefer remarks on some lesser orders (Mercedarians, Mathurins, Antonins) complete the text. There is a short, basic, but old bibliography with abbreviated references and a more useful index in which individuals, subjects, and places are included.

The reader is then on his own to leaf through more than one hundred plates of varying quality, grouped and identified by order, but lacking reference, in many cases, to the present location of the objects represented. Half of these illustrate paintings that range in date from a depiction of the *Coronation of Pope Celestine*, a provincial work in Hispano-Flemish style dated about 1525 (plate 27) to a stiff and heavily retouched portrait of Louise de Pardaillan de Gondrin, the last abbess of Fontevrault, ca. 1775 (plate 55). The remaining photographs, recording devotional statues, altars, retables, pulpits, stalls, ceilings, chapel decorations, and doorways are, on the

whole, the more interesting insofar as they indicate the context of a painting or relief, an essential element in the interpretation of works of art of the period.

The plates illuminate the pitfalls of the text: neither presents evidence of meaningful selection or arrangement. The founders of the orders receive the most consistent attention, but a paragraph mentioning the scenes in which St. Francis is depicted provides no better insight into the significance of his story than do the four illustrations chosen (plates 98, 100, 101, 102). More important, it is not at all certain that many of the works named were commissioned by or for the orders with which they are associated. But questions of patronage and provenience are not dealt with by Miss Evans. Neither is the issue of quality. It is intellectually confusing and esthetically offensive to find studies such as the sober painting of St. Jerome by Georges de la Tour (plate 114) or the poignant sculpture of *Our Lady of Sorrows* by Germain Pilon (plate 48) juxtaposed with such examples of kitsch as the gilt reliquary bust of St. Robert (plate 26) or the painting of St. Catherine of Siena (plate 96).

While *Monastic Iconography* does provide a usable, if costly, survey of the subject matter of late monastic decoration, anyone who anticipates an exploration of the meaning or development of the themes encountered, in the tradition of Erwin Panofsky, will find that that work has in no way begun.

LINDA SEIDEL
Harvard University

DAVID MALAND. *Culture and Society in Seventeenth-Century France*. (Studies in Cultural History.) New York: Charles Scribner's Sons. 1970. Pp. 319. \$8.95.

LEON BERNARD. *The Emerging City: Paris in the Age of Louis XIV*. Durham: Duke University Press. 1970. Pp. viii, 326. \$10.00.

David Maland and Leon Bernard have examined two important topics in the context of seventeenth-century French society. Although based on well-known books, Maland's is the more ambitious undertaking: he traces the literary, musical, and artistic developments as they emerge from the conditions and patronage of *la cour et la ville* (roughly the politics and society of noble courtiers and Parisian officers).

Bernard has focused his attention on Paris during what he describes rather broadly as the age of Louis XIV: in his own words this is a study of "the misty region between urban and social history," and it deals topically with the physical and architectural developments, the theater, transportation and communication, provisioning and education, lighting, cleaning and police services, as well as the guilds and the poor. The documentation for all the details consists of a wide variety of published sources and secondary books and articles. Comparison of the two books is inescapable, since they overlap in time, place, and subject. Maland displays a surer grasp of the technical aspects, and the political historian will find himself treated to a delightful and informative sketch of the culture of the *grand siècle*, while Bernard's readers will discover much about Parisian life of the 1600s without understanding what the Hôtel de Ville's city administration was like or how the new position of police lieutenant fitted into the old governmental patterns. Unfortunately, neither book succeeds in relating the subject matter to the sociopolitical context, but, on the other hand, the way each approaches that exceedingly difficult task should help other social historians see their way through some of the problems. Clearly, Maland has sought to strike a balance between using individual genius as an explanation for cultural trends and falling back on vague social and political forces. But, time and again, he has been drawn in one direction or the other. The author's decision to stress the classical elements of seventeenth-century French culture has, at one point, placed him in the camp of "national character" historians: not only does he state categorically that "French Catholics had too great a love of order, and too great a respect for the intellect, to approve the hysteria and the passionate mysticism to be found in Spain and Italy," but he concludes his argument by tracing that national tradition back to St. Bernard and Suger. It would have been more instructive to probe the seventeenth-century context, which, in the interpretation of recent articles by Robert Mandrou, becomes a highly complex and tragic sociocultural situation, not simply French "classicism." At the other extreme, the author finds no explanation for the fascination of the Le Nain painters with the poor, al-

though Pierre Deyon has explained that seeming deviation from a noble-dominated culture by bringing out the sacramental symbolism and religious charity in scenes of the poor dining. Certainly "culture and society" books need not force art into a rigid social-class mold along the lines of an Arnold Hauser; but Maland's less precise approach leaves the reader wishing that there were something in between. Bernard has avoided some of Maland's problems, but largely by letting the facts stand for themselves. After the introduction, the basic theme of the book—"how seventeenth-century Parisians dealt with the challenges resulting from . . . an explosive growth"—seems to be forgotten, except for occasional evaluations based on an undefined "modernity." In short, Paris in the age of Louis XIV does not appear very different from the capital of earlier or later decades, or other European cities of the period. One need not accept Orest Ranum's provocative picture of early seventeenth-century Paris (reviewed in *AHR*, 76 [1969-70]: 123-24), but it does have the merit of invoking a distinct image. Until we show how the complex social structure, existing institutions (including the still important Hôtel de Ville and courts such as the Parlement), as well as the experience of the Fronde affected Paris and Parisians, we will not be able to explore in depth the Parisian setting under the Sun King. At the same time, the superimposition of the old *déformation* theory of late Ludovician politics on Bernard's Paris (with 1789 appearing imminent) as on Maland's French culture ("pseudo-classical" and "degenerating" by 1715) makes one wish that political, social, and cultural history could be combined at the same level of sophistication.

A. LLOYD MOOTE

University of Southern California

PHILIPPE ERLANGER. *Louis XIV*. Translated from the French by STEPHEN COX. New York: Praeger Publishers. 1970. Pp. 412. \$10.00.

This is an excellent translation of a biography of Louis XIV that appeared in France in 1965 amid rather extravagant praise by press critics (a *Figaro Littéraire* panel termed it "the best work of history in France for a century") and a good deal less enthusiasm from scholars.

Erlanger's book must be judged for what it is meant to be: a very literate and entertaining narrative that does not shun the psychological insights and licenses of the novelist. The author intended it to be read by large audiences, and—at least in France—it has been. The bibliography would hardly be acceptable for an M.A. thesis; the documentation, what there is of it, is mainly Saint-Simon, Louis xiv's memoirs, Mme de Motteville, Dangeau, and such well-worn sources. Anecdotes and intriguing bits of information abound: for example, after the removal of his upper teeth, the king disconcerted his companions by passing liquids through his nose; Louis was so even tempered that he lost his composure only five times in fifty-four years; Fénelon was indignant because the new luxury of the times permitted common people to sleep in separate beds rather than the entire family crowding into one; the control of Elizabeth Farnese over Philip v was such that she even insisted their nightstools be placed side by side in a single alcove. Erlanger often reaches conclusions that seem to rest on no sounder foundation than his imagination. He credits Louis with "instinctively" perceiving the danger and preventing Colbert from transforming France into a twentieth-century "bureaucratic, socializing despotism." The Sun King was equally farsighted when, in the closing months of his reign, he was said to be striving for a triple alliance of France, Spain, and Austria, which would have, in Erlanger's view, prevented the rise of Prussia and Russia as first-rate powers and made it possible for the twentieth century to "avoid the cataclysms it has suffered."

LEON BERNARD

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JEAN SENTOU. *La fortune immobilière des Toulousains et la Révolution française*. (Commission d'histoire économique et sociale de la Révolution française. Mémoires et documents, Number 24.) Paris: Bibliothèque Nationale. 1970. Pp. 179.

Jean Sentou's *thèse principale* (*Fortunes et groupes sociaux à Toulouse sous la Révolution* [1969]) is a statistical study of the distribution of wealth at Toulouse in 1790, based on 5,369 marriage contracts and wills. Emphasizing the same quantitative approach in

this study—the use of magnetic tape and computer, logarithmic scaling, and the same classifications of social groups—Sentou's complementary thesis treats land transfers in the district of Toulouse from 1791 to 1800, based on 3,156 contracts of sale. Hence, Sentou is able to produce precise statistical data to answer the question posed forty-three years ago by Georges Lefebvre: which social groups bought land during the decade of the French Revolution? Sentou's work has the special merit of including not only the sales of national property (of the Church and the emigrés) but also private sales, which represented three-quarters of the value of the land transferred during the decade. Fortunately, few buyers appear more than once, suggesting that land speculation was at a minimum at least during this decade, and ninety-eight per cent of the land was sold to local residents, eliminating the problem of outside purchases.

The results of this study are presented clearly in tabular form, indicating annual transfers of land by social group, average individual purchases in deflated francs, and even "rates of enrichment" derived from the ratio of "net gain" (purchases over sales) to total purchases. It is impossible in a short review to furnish more than a sample of the findings. For example, the net loss to the local nobility accounted for 2,877,855 francs in land value, forty-two per cent of which was sold between 1796 and 1798, suggesting sale for financial needs rather than outright confiscation. Whether this constitutes the beginnings of an "inexorable" trend toward "noble impoverishment," however, is another question. The biggest buyers were the *negotiants* (wholesale merchants, not manufacturers) who made 601 purchases, or an average of 6,556 francs each, with the liberal professions, small commerce, and public services trailing behind. The *rentiers* of Toulouse actually registered a net loss during the Revolution. Sentou interprets this as a failure of the *rentiers* "to participate in the gigantic class struggle . . . for the control of landed capital."

Most striking is the "enrichment" of the artisans of Toulouse who made 566 purchases, averaging 3,679 francs each. This suggests that the artisans (building and luxury trades, *alimentation* and clothing) had sufficient capital

to make substantial investments in land during the Revolution. To me it seems misleading to characterize this "rise of the *petite bourgeoisie*" as part of a victory of commercial capitalism at the expense of the *classes populaires*. Other classifications of social groups for this period place the artisan—the prototype of the *sans-culotte*—in the *classes populaires*. But whatever the problems of interpretation, the data are there in voluminous, clear, and precise form to be consulted by social historians interested in how the French Revolution altered the distribution of land. At Toulouse the *boulevèrsement* was much greater than anyone had previously suspected.

ROBERT FORSTER

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JANINE MOSSUZ. *André Malraux et le gaullisme*. (Cahiers de la Fondation nationale des sciences politiques, Number 177.) Paris: Armand Colin. 1970. Pp. 312.

Jean Lacouture has said that Malraux was perhaps one of only two men who directly influenced "the life and mind" of Charles de Gaulle. Mlle Mossuz does not undertake to prove this proposition but rather that Malraux's Gaullism followed from his lifelong pursuit of human fraternity. It is her thesis that Malraux's thought has been unitary; that his passionate support of de Gaulle was perfectly compatible with his libertarian record in Indochina or Spain before 1939; and that ultimately Malraux celebrated and made use of the Gaullist myth as a foundation from which he could try to make apparent the still grander cultural myth (hitherto masked by religion and ideology) through which all men may yet recognize their fraternity.

It is a large undertaking, based on a reading of the novels, the "anti-memoirs," and particularly the speeches (of which many texts remain unpublished) delivered in France and around the world over the last quarter century. Most of her book deals with Malraux's activities as defender and pageant-master of the ill-fated RPF and then of the Fifth Republic. Certain conclusions are evident: his political ideas were utterly vague; his program during the years spent "crossing the desert" (that is, the Fourth Republic) had one goal—bring the General back to power and all will be well;

reason had no place in his outpourings; he was interested less by politics than by "history"; and where Malraux as minister could not accept what was being done (for example, the army's use of torture in Algeria), he kept quiet, presumably in the interest of the greater end. All this is fully presented, buttressed by a mass of quotations, perhaps even excessively. But it comes as a slight shock for the author to have suggested that, while serving the Gaullist myth, Malraux may have been dreaming of himself becoming the "grande figure" of the larger "nouveau mythe réunificateur." Fact or fancy, however, it does not detract from the value of this discussion of his thought and action.

Whether the flamboyant, apocalyptic harangues by the RPF militant and later minister for cultural affairs justify so elaborate an examination is an open question. They certainly alarmed such cabinet colleagues as Jacques Soustelle, who was happy to see Malraux kept traveling to Pnom Penh, São Paulo, Quebec, or Washington. What ordinary Frenchmen (hoping for word on the cost of living or new housing) made of his darkly prophetic orations, ranging disconcertingly back and forth across history, one must guess. Alexander Werth once wrote that he doubted whether de Gaulle took Malraux's "historical gibberish" seriously; he was probably mistaken. Malraux's thought struck a chord in the General's mind, where mysticism and cynicism jostled each other. Moreover, Malraux lent a certain cachet to a succession of otherwise merely hard-faced cabinets. And while evidence for the study of the regime accrues, one may be grateful for this thoughtful assessment of "the bard of Gaullism."

JOHN C. CAIRNS

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WOLF MENDEL. *Deterrence and Persuasion: French Nuclear Armament in the Context of National Policy, 1945-1969*. (Studies in International Politics.) New York: Praeger Publishers. 1970. Pp. 256. \$9.00.

In the 1950s, before de Gaulle returned to power and in defiance of frowns from Washington, the French government decided to equip its military with atomic weapons. The way in which France entered the game of

nuclear armaments provides the theme for Professor Mendl's study.

France did not immediately embark upon a military program of nuclear development in the postwar years. The British and American governments made it clear that they were unwilling to share their secrets with France, and the costs of building a plant capable of producing U-235 would have overtaxed the resources of a government that faced heavy demands for social and economic reconstruction. Nevertheless, research into the military uses of atomic energy began in the early 1950s, and with the launching of a second five-year plan for nuclear development in 1957, military goals had taken precedence over development for industrial purposes. After de Gaulle's arrival in power, the military program was pushed ahead rapidly, although basic decisions had been reached under the Fourth Republic.

The driving force for nuclear armaments came from a small group of *Polytechniciens* who obtained support from key politicians in the 1950s. The military had a lesser role in the decision-making process, for many officers were distracted by colonial wars and by their participation in NATO. There was little public debate over expenditure for nuclear armaments. By the time the issue came into the open, a series of behind-the-scenes decisions had already committed France to a military program.

The technocrats who promoted French nuclear development were motivated primarily by considerations of foreign policy. In this realm the possibility of a French threat to the Soviet Union was less important than strengthening France in relation to her Western allies. France's nuclear force was intended to give the government greater authority in dealing with the Anglo-Saxon powers, and it also would make France the dominant partner in any agreement with Germany. Yet Mendl concludes that the program of nuclear armaments has been less successful in strengthening French foreign policy than its proponents had predicted. France's status has improved during the past fifteen years, but Russia and the United States still have not admitted France as an equal even in European affairs, and Germany's relationship with France seems unrelated to French nuclear capabilities.

Despite a tendency toward repetition and the absence of a clear theoretical framework, which might have given greater precision to his discussion of the technocratic aspects of the decision-making process, Mendl has provided a useful guide to the motives behind France's postwar nuclear policy.

J. K. MUNHOLLAND

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JOSÉ C. NIETO, *Juan de Valdés and the Origins of the Spanish and Italian Reformation*. (Travaux d'Humanisme et Renaissance, Number 108.) Geneva: Librairie Droz. 1970. Pp. xvii, 355.

Professor Nieto's first book, his doctoral dissertation, is an important one. Rethinking extensive secondary and primary materials, he has conceived a new synthesis that all scholars working on Juan Valdés will have to consider. On Spain and Italy in the Valdés period readers will continue to turn to the books of Marcel Bataillon and Frank Church, but they will have to take account of Nieto's discoveries. On Valdés himself one must begin with Nieto. Even more significant is the new approach Nieto suggests to the whole early Reformation period.

The most interesting part of the book, approximately half of its length, provides an analysis of Valdés' theology. Nieto argues that "Valdesianism" represents an indigenous Spanish position, largely independent from Northern influences, either Erasmian or Lutheran. The main influence on Valdés' thought was, rather, Pedro Ruiz de Alcaraz, here distinguished among the *alumbrados* of early sixteenth-century Spain as taking a basically theological rather than mystical position. Emphasizing the concepts of religious "knowledge" and religious "experience," Nieto shows a position similar to Lutheranism yet significantly different from it. This establishment of a Southern base of origin not only provides a new understanding of the whole new theology as a product of Europe-wide influences but also helps to clarify the role and impact of the Northern reformers.

Nieto's thesis will inevitably require some digesting, and even further investigation, before it becomes the canon. I am convinced by his argument, yet it may be useful to suggest future *points d'appui*. Nieto alters Valdés'

traditional birthdate and shows him writing his first book, the *Diálogo de doctrina christiana*, before he was eighteen, when he entered the University of Alcalá and came under Erasmian influences. This seems precocious, especially as Nieto claims that the book incorporated *in nuce* the whole of Valdés' later theology and was subtle enough to fool the Erasmians at Alcalá. Similarly Nieto's emphasis on the importance of this first book tends to overshadow the problem of Valdés' subsequent theological development, particularly interesting if the *Diálogo de doctrina* was the product of extreme youth. Nieto fills important lacunae in Valdés' Neapolitan phase from testimony taken twenty-five years after his death at the inquisitional trial of Cardinal Pietro Carneseccchi. The Council of Trent had occurred in the interim; certainly this material must be treated with extreme caution. I do not intend to suggest that Nieto is ever less than careful in handling his materials, but rather that the problem should now be thought through with special emphasis on Valdés in Italy.

PAUL STEWART

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BARTOLOMÉ BENNASSAR. *Recherches sur les grandes épidémies dans le nord de l'Espagne à la fin du XVI^e siècle: Problèmes de documentation et de méthode.* (École Pratique des Hautes Études—VI^e Section. Centre de Recherches Historiques. Démographie et sociétés, Number 12.) Paris: S.E.V.P.E.N. 1969. Pp. 194. 33 fr.

As the title of this slender volume suggests, it is not a study of the epidemic of bubonic plague that hit Spain in 1596–1602 but a detailed proposal for such a study. M. Bennassar indicates some of the questions and answers suggested by documents he has encountered in his other researches, discusses the familiar problem of examining Spain's widely dispersed historical sources, and offers some very tentative hypotheses as to what the study may reveal. About half of the volume is devoted to reproducing with brief analyses the two principal sources actually examined: the relevant debates of the city council of Bolbao and the reports of a number of royal *corregidores* regarding the course of the plague in various cities.

The format proposed for the final study will

be familiar to readers of the author's *Valladolid au siècle d'or* (1967) and other publications of the Sixième Section of the École Pratique des Hautes Études. As others have done, M. Bennassar presents the plague as a major factor in the final termination of Spain's demographic-economic expansion in the sixteenth century. The epidemic is treated as a strategic part of a model for long-run economic trends derived from the work of Roger Mols, René Baehrel, Antonio Dominguez Ortiz, and others. There is no mention, however, of the work of B. H. Slicher van Bath in applying such models.

Overall, the most interesting aspect of M. Bennassar's hypotheses is his belief that the incidence of the plague was much heavier and more widespread than thus far assumed by historians. If he can prove the amount of destruction he suggests, the epidemic of 1596–1602 would truly become the first of the "three great offensives of death" against seventeenth-century Spain that Antonio Dominguez ably outlined several years ago. The result of such a finding might be a real clarification of the stagnation and decline of the Spanish interior in the early seventeenth century.

DAVID R. RINGROSE

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SUNE JUNGAR. *Ryssland och den svensk-norska unionens upplösning: Tsardiplomati och rysk-finländsk pressopinion kring unionsupplösning från 1880 till 1905.* (Acta Academiae Aboensis, Series A, Humaniora: Humanistiska Vetenskaper; Socialvetenskaper; Teologi, Volume 37, Number 3.) Åbo: Åbo Akademi. 1969. Pp. 197.

Most scholars who have studied the dissolution of the Swedish-Norwegian union in 1905 have been mainly interested in the course of events within the Scandinavian peninsula itself. As far as outside reactions to the breakup of the union are concerned, the policies and attitudes of Germany and Britain have also received attention, while non-Russian historians have little explored Russian views on the matter until the appearance of the present study. The author approaches his task basically on three levels. Besides discussing Russian diplomacy and the opinions of the Russian press in

regard to the crisis in Scandinavia, he also analyzes Finnish responses to it in what almost amounts to a separate little study of its own.

What, then, are the author's conclusions? At the height of the union crisis Russia was preoccupied with war with Japan and internal turmoil and could not take a strong stand in Northern Europe, especially because of the loss of its Baltic fleet in Tsushima; but the outcome of the Scandinavian affair nevertheless coincided with the desires of St. Petersburg. Both the Russian government and most of the press were pleased with what they considered to be weakening of a hostile Sweden. This was to be expected since it has been a persistent feature of Russian foreign policy to keep Scandinavia divided, irrespective of ideological preferences. Ideologically the tsarist regime felt more affinity to the conservative Swedes than to the radically democratic Norwegian separatists. But for reasons of power politics and national interests Russia was quite willing to see the latter have their way. Conversely, the equalitarian spokesmen of Norway were quite ready to agitate for their cause in conservative St. Petersburg newspapers. German efforts to persuade Russia to lend common support to the Swedish dynasty against the Norwegians, anti-Russian publications in Sweden, and Swedish sentiments toward Russification policies in Finland simply convinced the tsarist regime of the need to weaken Sweden: its neutrality leaned too far toward Germany to appear credible to the Russians.

In Finland ideological preferences and considerations of its standing in the tsarist empire, rather than international power politics, tended to determine views. The Socialists and liberals, both among the Finns and Swede-Finns, favored Norway's cause, while the more conservative elements took a dimmer view of its struggle. All factions in Finland admired, though, the unity of the Norwegians in the showdown, being very much aware of their own difficulties in closing ranks when facing Russification.

In analyzing the reasons for the differing views of the various Finnish political groups, Jungar appears somewhat puzzled with the reactions of the Old Finns. Perhaps their attitudes toward the Norwegians were due to their sensitivity to or fear of Russian suspi-

cions concerning Finland as well as to their ideological inclinations. The Old Finns may have realized that although pro-Norwegian statements in the Russian press suited the tsarist regime, similar statements originating from Finland might have been interpreted differently.

Jungar has made thorough use of the sources available to him. He did not gain access to all the Russian material that could have bearing on his topic, through no fault of his; he had to face the customary difficulties confronting foreign scholars working in Soviet archives. This is a clearly written, persuasive, and solid study and an important contribution to the history of Northern Europe.

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HANS-DIETRICH LOOCK. *Quisling, Rosenberg und Terboven: Zur Vorgeschichte und Geschichte der nationalsozialistischen Revolution in Norwegen.* (Quellen und Darstellungen zur Zeitgeschichte, Number 18.) Stuttgart: Deutsche Verlags-Anstalt. 1970. Pp. 587. DM 52.

Readers of the correspondence columns of the *Times Literary Supplement* may recall three lively exchanges on the vexatious problem of Vidkun Quisling. The first round, in April and May 1965, revolved about a biography, *Quisling: Prophet Without Honour* (1965), by Ralph Hewins, which various critics denounced as entirely too favorable to its subject. In January 1970 Mr. Hewins drew a sharp retort from Magne Skodvin of Oslo University for suggesting that the word "quisling" be dropped from dictionaries. Round three began in January 1971 with another letter from Hewins, who referred to official investigations begun in Norway in September 1970 as to the legal status of the country during the period of German occupation. Hewins also mentioned *Quisling, Rosenberg und Terboven* by Professor Hans-Dietrich Loock of Berlin, "which was reviewed at length in *Der Spiegel* on September 7, with copious repercussion in Norway, including the news that his inspiration in this work was none other than the controversial Professor Skodvin." Hewins concluded by indicating dissatisfaction about the "curious academic situation" of the investiga-

tion. No doubt we shall hear more of this later.

Loock's book—which actually had its origins more at the Institut für Zeitgeschichte, Munich, and the Friedrich-Meinecke-Institut, Berlin, than at Oslo—is a fair and solid contribution to a complicated and gloriously controversial subject. It begins with an account of Quisling's early career and murky Weltanschauung, which was a mixture of universalism, anti-Bolshevism, Nansen-style idealism, and Nordic *tull*. Loock gives scant attention to conventional biographical details and makes little attempt to explain personality and motivation. Dismissing Hewins' book as "unusable for scholarly purposes," he confines his own to external actions, so that one gets little sense of a progression of ideas. By the middle 1930s Quisling, fearful that in a German-Soviet conflict the Norwegian Labor party would open the doors to the Red Army, was a rather forlorn figure, proclaiming the slogan "Nordics of the world, unite!"

At this point Loock brings in his second protagonist, Alfred Rosenberg, whose *Myth of the Twentieth Century* was published in 1930, the same year as Quisling's *Russia and Ourselves*. Despite some intellectual differences the two visionaries and "new chiliasts" had much in common. At the end of 1938 Hans Wilhelm Scheidt brought "a new north wind" to the Aussenpolitisches Amt of the NSDAP, and partly through his influence Quisling "crossed the Rubicon" in June 1939 to work directly with Rosenberg. Loock does not explain how the Nazi-Soviet Pact affected the two anti-Bolshevist thinkers, but he indicates that it took Grand Admiral Erich Raeder to get things moving. Loock portrays Quisling and Albert Viljam Hagelin, who saw Hitler in Berlin in December 1939, as the jokers in Raeder's game for the occupation of Norway.

Taking expert advantage of the rich materials available, published and unpublished, Loock places his main emphasis on the tangled events of 1940: British and French intentions; Hitler's grandiose plans for a Germanic Reich; the formulation and execution of Operation *Weserübung*; problems faced by the Norwegian authorities; Quisling's ineptitude as a politician; questions of legitimacy, "legal revolution," and resistance; and the organization of civil administration. Some of the most interest-

ing and revealing pages are those on power struggles among the Germans: Curt Bräuer, the correct and hapless minister in Oslo, not really supported by Ribbentrop; Scheidt, representing Rosenberg; Josef Terboven, Gauleiter of Essen, who became Reichskommissar of Norway supposedly with the backing of Goering and to a lesser degree of Himmler; and Raeder, who maneuvered Quisling's restoration after Terboven had forced him out. Bureaucratic intrigue, notably the jostling of Nazi leaders to gain Hitler's support, makes a fascinating story. Loock's conclusion, that no one was satisfied with the arrangements worked out in September 1940, is borne out by a short afterword carrying the story on to the liberation of Norway in 1945.

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HERBERT JANKUHN, with the collaboration of HARALD JANKUHN, EBERHARD MAY, and ULRICH WILLERDING. *Vor- und Frühgeschichte: Vom Neolithikum bis zur Völkerwanderungszeit*. (Deutsche Agrargeschichte, Volume 1.) Stuttgart: Verlag Eugen Ulmer, 1969. Pp. 300. DM 46.

This book titled "Pre- and Early History from the Neolithic to the Migration Period" is volume 1 of the five-volume series dedicated to the history of agriculture in Germany. Prepared by Professor Herbert Jankuhn, it deals with the economic basis of prehistoric and early historic Central Europe and includes invaluable surveys of "The Beginning and Development of Cultivated Plants in Pre- and Early Historic Times" by Dr. Ulrich Willerding, "The Beginning and Development of Domestication of Animals" by Dr. Eberhard May, and "Linguistic Evidence for the Earliest History of Agriculture" by Dr. Harald Jankuhn. The appendixes, informative and up to date, are the most concise introductory papers on worldwide domestication of plants and animals and of Indo-European linguistic paleontology. The main treatise presents the evidence for the reconstruction of economic and social bases in Germany by period—the Neolithic, Bronze, Iron, and Roman.

Herbert Jankuhn speaks not only about agriculture, agricultural tools, domestic and wild animals, but also about the settlement

patterns, architecture, flint mining, crafts, trade, communication, and social differentiation. In other words the reader will find in this book useful information about pre- and early history without having to go through the usual analysis of artifact forms and discussions on chronology.

The book is a response to the needs of recent archeological research and is based on the accumulated materials and results of settlement archeology assisted by studies of geologists, paleobotanists, paleozoologists, and others. Radiocarbon dating is accepted by all the authors. Chronologically, Jankuhn's survey begins with the Linear Pottery culture of the fifth millennium B.C. and ends with free Germania in Roman times. Spatially, the author had to limit himself to the present area of Germany, which in prehistory never coincided with a cultural zone or an ethnic unit. Therefore he derives most of his information from Germany but much of what he says also pertains to upper Danubian and Baltic Europe between the Rhine and Denmark on the west and northwest, Switzerland and Austria on the southwest and south, and Hungary and Poland on the east.

This book supplements J. D. G. Clark's *Prehistoric Europe—The Economic Basis* (1952), which by now is out of date. Fundamental works on domestication of plants, animals, new chronology, climatic history, and other vital questions are yet to appear. Regional summaries such as this are welcome and highly recommended for students of Central European pre- and early history. Appendixes on plant and animal domestication and on linguistic paleontology are convenient summaries that may be used as textbooks.

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HUGO STEHKÄMPER, editor. *Der Nachlass des Reichskanzlers Wilhelm Marx*. In four volumes. (Mitteilungen aus dem Stadtarchiv von Köln, Numbers 52–55.) Cologne: Verlag von Paul Neubner. 1968. Pp. xii, 520; vii, 490; vi, 427; xix, 559.

During the first two decades after the death of Wilhelm Marx in 1946, his large and unorganized accumulation of papers was limited in both usefulness and access. In 1964 the

Stadtarchiv von Köln closed the materials completely and undertook a thorough analysis and ordering of the entire collection. The result of these efforts is recorded in these four volumes of meticulous detail; an index volume will appear shortly.

Marx represented the more modern and liberal wing of the Catholic Center party. Beginning his work at local party levels in the Rhineland, he rose through the old Prussian Landtag and imperial Reichstag, participated in the Weimar Assembly and Reichstag, and served four terms as chancellor of the Weimar Republic between 1923 and 1928. In his time he was upstaged by Gustav Stresemann's highly visible activities as foreign minister. Subsequently he was overshadowed by the more dramatic struggles of his successor, Heinrich Brüning, in the final crises of the Republic. Yet Marx deserves more adequate recognition as a political figure of talent, accomplishment, and appeal. After all, a shift of just half a million votes would have made him president of Germany in 1925 in the coalition runoff elections against Hindenburg.

The papers contain no sensational revelations, but they are a major source for the study of German internal history between 1890 and 1930. Among the more important items are twelve hundred typed manuscript pages of memoirs, a long run of shorthand calendar notations, detailed documentation on local and national Center party deliberations, Marx's personal library of books and pamphlets (some annotated), a newspaper clipping file of 75,000 items, and a fine collection of pictures and portraits.

German history specialists will find these archives particularly useful for study of developments in German political party structure and function from the Empire to the Republic. They offer a penetrating view of the social, cultural, and political characteristics of German Catholicism. Some light falls on the revolution of 1918–19 and the Weimar Assembly. There is broad documentation of Marx's interest in various aspects of education. Two problems of his years as chancellor are particularly well documented—the reparations questions and threats of separatism in the Rhineland.

Given our current fascination with ideology,

search for alleged relevance, and haste for change, the work of the trained archivist is likely to be overlooked. For those who would be reminded how much we historians must depend upon the work of archivists, I strongly recommend the introductory chapter of this series. These thirty pages give a succinct account, step by step, category by category, of master craftsmen at work, ordering and systematizing a vast array of materials in order to create the documentary basis for subsequent research by other historians.

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CHARLES H. O'BRIEN. *Ideas of Religious Toleration at the Time of Joseph II: A Study of the Enlightenment among Catholics in Austria.* (Transactions of the American Philosophical Society, New Series, Volume 59, Part 7.) Philadelphia: the Society, 1969. Pp. 80. \$2.50.

Professor O'Brien has written a judicious interpretation of the Edict of Toleration issued by Joseph II in 1781. The edict was the first charter of religious toleration that extended to Calvinists, Lutherans, and Eastern Orthodox throughout the Habsburg Empire the right of private religious worship and recognized their corporate status as distinct church organizations. Supplementary edicts defined the right of Jews to enter educational institutions and to practice trades, crafts, and the professions. In his detailed and well-documented study the author is particularly attentive to the special circumstances of eighteenth-century Austria that explain the edict's publication in 1781. In addition he argues convincingly that the Josephinist formula of religious toleration was an example of a compromise solution, inspired principally by an enlightened Catholic viewpoint and not at all expressive of the eighteenth-century secular, anti-Catholic Enlightenment, as has sometimes been argued erroneously.

In support of his thesis the author cites the emperor's educational and religious reforms and his censorship policies. These were designed to encourage Catholic acceptance of toleration through reforms in seminary training and religious instruction and by promoting an enlightened Catholic religious litera-

ture. From the author's analysis of Austrian discussions of religious tolerance in the 1780s it is evident that the edict received primary support from enlightened Catholic clerics, such as Bishop Leopold von Hay and Joseph von Auersperg, while secular-minded humanists, notably Joseph von Sonnenfels and Voltairians Johann Pezzl and John Baptist Alxinger, found the edict inadequate.

The principal shortcoming of Professor O'Brien's treatment is his narrow focus on religious toleration and his neglect of its relationships to Josephinist state centralism. The reader is not made sufficiently aware that the edict was only one aspect of Josephinist political reforms. Nor is it mentioned that religious toleration had important yet differing relationships to political reforms in various parts of the empire. Apart from this point, however, Professor O'Brien has composed an important study in modern Austrian history.

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PETER SCHUSTER. *Henry Wickham Steed und die Habsburgermonarchie.* (Veröffentlichungen der Kommission für neuere Geschichte Österreichs, Number 53.) Vienna: Verlag Hermann Böhlau Nachf. 1970. Pp. 208. DM 34.

The subject of this published doctoral dissertation is the evolution of the anti-Habsburg stance of Henry Wickham Steed (1874-1956), the influential London *Times* correspondent in Vienna from 1902 until 1913 and later foreign editor of the *Times*. Steed, according to the author, played no small role in bringing about the breakup of the Habsburg monarchy by his energetic support of the political programs of anti-Habsburg emigré groups—above all the South Slavs and the Czechs—during the First World War. Schuster's thesis—neither new nor perceptively developed—is that Steed's anti-Habsburg position was grounded in his obsessive belief in Germany's ambition to establish its hegemony in Europe and the Near East. As long as Steed believed that Austria-Hungary, by acting independently of Germany, could provide resistance to the "German danger," Steed's attitude toward the monarchy, even when critical, was sympathetic. When, after the annexation of Bosnia and Herzegovina, he concluded that it was

impossible for Austria-Hungary to pursue an independent policy, his attitude became hostile, and he sought to build resistance to German expansion by supporting Slavic nationalist movements within the monarchy.

Schuster's study is based primarily on Steed's published works: dispatches to the *Times*, memoirs, numerous articles, and the widely read book, *The Hapsburg Monarchy*, first published in 1913. Steed's papers did not yield much, since most of Steed's correspondence with the *Times* is deposited in that newspaper's archives, to which Schuster was unable to obtain access. An uncompleted manuscript revision of Steed's memoirs, written a few years before his death, which Schuster found among the journalist's papers, shows that Steed's views remained consistent with those contained in the memoirs published in 1924. In addition Schuster has relied heavily on the relevant volumes of the *History of the Times* and on published diplomatic documents. A small amount of unpublished Austro-Hungarian and British foreign office material drawn from the Vienna Haus-, Hof-, und Staatsarchiv and the Public Record Office in London provides some pertinent details and glimpses into the press relations of the two foreign offices.

Schuster's study contributes little to our knowledge or understanding of Steed or of the sociopolitical context in which he acted. Certainly there is much to criticize in Steed's restrictive and moralistic Whig interpretation of Habsburg history, his anti-Semitism, and his underlying arrogant assumption—shared by many inhabitants of Fleet Street and the Foreign Office—of the political and moral legitimacy of British imperial hegemony. On the other hand Steed was not entirely wrong about the internal and external imperatives created for the Habsburg monarchy by its oligarchical and dynastic sociopolitical structure or about Germany's power ambitions. Schuster's narrative and interpretive framework, however, based on narrowly drawn categories of political and diplomatic history, which are the same as Steed's and even more narrowly conceived, cannot illuminate the significant events in which Steed participated or the important historical issues to which he is linked. The structure and tone of Schuster's

study are closer to pre-World War II German and Austrian historiography, with its polemics over the "war-guilt question" and the "dictated Treaties of Versailles and St. Germain," than to recent historical scholarship, which attempts to see the collapse of the Central and East European empires as occurrences within long-term processes of economic and social change. It is this constricted framework and not Schuster's pro-Habsburg bias that diminishes the value of the study. Missing from the bibliography are, among others, the names of Fritz Fischer, Otto Brunner, and Vladimir Dedijer, as well as all East European historians of the post-1945 period.

One final comment: the chapters are subdivided into sections that are then further subdivided, a format that gives the reader a feeling of reading note cards.

SOLOMON WANK

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GIORGIO GALLI and ALFONSO PRANDI. *Patterns of Political Participation in Italy*. New Haven: Yale University Press. 1970. Pp. xviii, 364. \$15.00.

For all serious scholars and students in the field of Italian contemporary history this work is a prerequisite for achieving a more realistic appraisal of Italian political, social, and economic phenomena. According to the authors, in recent years nothing has been done to free the history of Italy from the myth of a stereotyped American and Anglo-Saxon cliché. In the last two decades the history of Italy has been shrouded by foreign historians like René Albrecht-Carrié and Denis Mack Smith who have implied that the continuity of Italian history is "unbroken in both its liberal and fascist periods" and have portrayed the Italians as having an "inherent inability to adjust to a free society." Fortunately the present work is available to American scholars and students in the field of Italian history. It contributes mightily to the process of demythicization of arbitrary and biased views.

The purpose of the book is threefold. First, it gives a new view of and a deeper insight into Italian socioeconomic and political problems. It announces that the wealth of information and statistical data—collected between 1962

and 1966 by the team of experts under the supervision of the Istituto Studi e Ricerche "C. Cattaneo" of Bologna—is available to scholars and students of other countries. Second, the book proves unmistakably, and in my opinion for the first time, that Italian political and economic problems cannot be studied in isolation. In their brilliant analysis the authors prove that in time of communal, provincial, and governmental elections, international events influence Italian political life. In 1958, for instance, though the political campaign per se had no central theme, the Twentieth Congress of the Soviet Communist party, the de-Stalinization process, the Hungarian revolt, the American recession, and the Algerian coup of May 13 were instrumental in maintaining in Italy a static political situation without any unpredicted change. Third, the authors with acute and almost rare historical perception interpret the evolutionary process of Italian democracy from the experience of Centrism to the Center-Left formula. To a certain extent the contradictions of the Italian political and socioeconomic system ("a dynamic society with a static political system") that exploded during the period of Centrism started to bridge out during the progressive era of the Center-Left opening. Therefore, in line with the events of the last decade, the authors affirm that the Center-Left formula must be seen as the result of the conquest of power by those popular forces that had been left out of the state of the Risorgimento, that is to say both the Catholics and the Socialists, as truly democratic and popular expressions. The exclusion of the Liberals from the government coincided with the end of the outdated doctrine of the liberal state and the formation of a new and democratic one, open to the principles of social justice, political effectiveness, and popular participation.

The book by Giorgio Galli and Alfonso Prandi is so promising that one can only hope that scholars and students from now on will feel encouraged enough to bring their interpretations of Italian events down to a more realistic sense of history.

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ALAN CASSELS. *Mussolini's Early Diplomacy*. Princeton: Princeton University Press. 1970. Pp. xvii, 425. \$12.50.

This is a very satisfying monograph, clearly focused on the topic, the product of very extensive research, well thought out, and well written.

Cassels distinguishes four subperiods of early Fascist diplomacy: the first six months, largely taken up with problems left over from World War I; July 1923 to May 1924, when the Duce began asserting himself with the occupation of Corfu and outright incorporation of Fiume, with secret dealings with the German nationalists, and with plans for a war against Turkey; a conciliatory phase, June 1924 to April 1925, under the shadow of the Matteotti murder; and May 1925 to February 1927, when establishment of the dictatorship freed Mussolini's hands for a more forceful policy. A subplot runs through the narrative—the failure of the professionals in the foreign ministry, led by the secretary general, Salvatore Contarini, to guide Mussolini back into traditional paths, a failure marked by the departure of several prominent diplomats and the appointment of Grandi as undersecretary. Not all of this is new, but the documentation and balance offer a contrast to Salvemini's strictures, which, as the author states, fail to give the devil his due by depicting him as a buffoon.

Cassels refutes the myth of a contrast between Fascist foreign policy of the thirties and of the early period when Mussolini was said to have been "a sound and useful leader, no more aggressive in his nationalism than many a democratic statesman." He shows that Corfu foreshadowed Mussolini's defiance of the League in 1935; that the groundwork for occupation of Albania and of Ethiopia was laid in the early period; that the Duce's secret dealings with the Nazis and aid in the clandestine rearmament of Germany portended the Rome-Berlin Axis.

The author stresses that the entente with Britain was largely responsible for the good opinion held of the Fascist dictator. The British Conservatives projected the favorable image abroad, particularly in the English-speaking world. Furthermore, this entente "colored the historical judgment" of Mussolini's early diplomacy, and a footnote lists a dozen honored members of the historical profession who

have been taken in by the myth of "a decade of good behavior."

If fault be found with this excellent study, I suggest that many of the footnotes are too cryptic. A reference to a document should give its date and kind (telegram, memo, letter, or dispatch) rather than indicate simply where it is to be found.

HOWARD MCGAW SMYTH
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NIKOLAJ TODOROV, editor. *La ville balkanique, XV^e–XIX^e ss.* (Studia Balcanica, Number 3. Académie Bulgare des Sciences, Institut d'Études Balkaniques.) Sofia: Éditions de l'Académie Bulgare des Sciences. 1970. Pp. 199. 2.50 L.

The fruit of an international colloquium organized in Moscow in the spring of 1969 by the Committee for Balkan Economic and Social History of the Association Internationale d'Études du Sud-est Européen (AIESEE), this publication represents the first systematic attempt to deal with Balkan cities in the Ottoman and preindustrial era. It consists of contributions by several scholars—one American, one Hungarian, two Turks, two Romanians, three Soviets, four Yugoslavs, and five Bulgarians.

The articles by Desanka Kovačević (Yugoslavia), Vasilka Tŭpkova-Zaimova (Bulgaria), Radovan Samardžić (Yugoslavia), Marta Bur (Bulgaria), Josef Perényi (Hungary), and Olga Ziroević (Yugoslavia) unintentionally constitute a unit and should be read in a precise order, starting with the Kovačević study, which depicts a concentration of Ragusan merchants in the rich mining towns of Serbia and Bosnia during the first half of the fifteenth century. After the decline of these mines the Ragusans sought out commercially more inviting places. Around 1490, according to Tŭpkova-Zaimova, they appeared in fairly large numbers in Bulgarian towns. With the extension of the Ottoman Empire's economic space by its conquest of Hungary (1541), relates Samardžić, a wealthy Ragusan merchant colony was developed in Belgrade. By the end of the century, however, it began to languish, and as Perényi shows, Orthodox Serbians, Moslem Bosnians, and other non-Hungarian elements extended their control over the urban economy of Hungary. The Samardžić and

Perényi articles concur with Marta Bur's study of Hungarian travel literature in discovering an intensification of traffic on the Danube during the second half of the sixteenth century. A proliferation of river piracy then ensued, but none of these authors raises the question of a possible connection between the growing insecurity of travel by river craft, which was accompanied by a recrudescence of Mediterranean piracy, and the gradual preference that was probably given to travel by land. On land, too, there were bandits, but the Ziroević article suggests a factor that may have enticed travelers to the land route between Buda and Pirot: the palanka, a bulwark or stockade of tree trunks, stakes, and earthworks at which was stationed a garrison and which generally included a small settlement, a wooden mosque, and several caravansaries and bakeries. Two systems of palankas came into existence from about the middle of the sixteenth century, one along the Ottoman Empire's northern frontiers and the other stretching southward from Buda (together they formed a "T"). The first constituted a defense against foreign enemies. The second sought to secure the line of communications with the capital against banditry and may have convinced long-distance merchants to take it in preference to the river route.

A second group of articles concerns the material life of Bulgarian towns, with papers by Maria Kalitsin on dress, Georgi Kozhukharov on urban architecture, and Nikolai Todorov—who was very active as chairman of the Committee on Social and Economic History—on housing. Todorov gives us a study of Vidin, Ruse, and Sofia that is well documented and shows that three-quarters of the houses in eighteenth-century Vidin contained no more than one or two rooms plus a kitchen, while houses in the other two towns were often without a separate kitchen.

Sergei A. Nikitin and Marlen Freidenberg (USSR) emphasize the rural character of Balkan towns, the first in a paper on Bulgaria in 1879 and the second in one on Dalmatia (on other Croatian and Dalmatian aspects, see Nada Klaić) three or four centuries earlier. Despite the sixteenth-century urban demographic growth (Ömer L. Barkan), Ottoman towns retained their "preindustrial" character,

which was reinforced in part by the hostility of craft guilds to competition (Halil Inalcik).

Valentin Georgesco and Constantin Șerban (Romania) identify an urban type intermediate between the Balkan and Central European models: Wallachian and Moldavian towns. A further call for comparative study appears in Yakov A. Levitsky's (USSR) survey of medieval Western European urban development. The one general study of Balkan towns was my own and provoked a lively discussion (see the discussion section). The Byzantinist and president of the AIESEE, Franjo Barišić, said of it that the "effort to give us an overall view, to place the problem of the Ottoman city in a vaster geohistorical framework [is] not premature. Based on a thorough and detailed study, the communication . . . suggests and traces new paths of research, introduces and stimulates new precisions in terminology" (p. 197).

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M. LACKÓ. *Arrow-Cross Men, National Socialists 1935-1944*. (Studia Historica Academiae Scientiarum Hungaricae, Number 61.) Budapest: Akadémiai Kiadó, 1969. Pp. 112. \$6.00.

NICHOLAS M. NAGY-TALavera. *The Green Shirts and the Others: A History of Fascism in Hungary and Rumania*. Stanford: Hoover Institution Press, Stanford University, 1970. Pp. xii, 427. \$9.95.

These two books, one about Hungarian fascism and the other about Hungarian and Romanian fascism, might appear to have been published at almost the same time, but in fact Lackó's work is a quaintly worded English version of the book he published in Hungarian in 1966. Lackó covers a much narrower field than Nagy-Talavera and makes far greater use of unpublished documents, in particular those connected with the war-crimes trials of the Fascist leaders. Nagy-Talavera's study, on the other hand, contains little original material, and where the author deals with fascism in Hungary he relies very heavily on Professor C. A. Macartney's *October Fifteenth: A History of Modern Hungary 1929-1945* (1956). Fully a third of his 600-odd footnotes cite Macartney, even though some of

Macartney's data must be modified or reinterpreted in the light of the findings of research done since they were first published.

Despite the greater originality of Lackó's sources, however, Nagy-Talavera's interpretations are more convincing. A major interest of both authors is the sociopolitical basis of Fascist movements. Lackó is at pains to persuade his reader that the Hungarian Arrow-Cross party contained only "confused" laborers and "distorted characters" (p. 38), the "most backward" elements of the proletariat (p. 45). He pursues this argument right through his book, but he is scholar enough not to shy away from his conclusion about the results of the 1939 elections: "Yet all this is by no means intended for denying the fact that a considerable part of the votes for the Arrow-Cross came from the class of the workers" (p. 72). Nagy-Talavera is, of course, correct to state plainly that the main strength of the Arrow-Cross came from the ordinary working class (pp. 141, 151, 235).

The books also differ in their presentation of the scope of Hungary's Fascist movements. Here Nagy-Talavera's approach is original. Lackó, while scanning the whole of the Arrow-Cross movement, concentrates on the factions led by Ferenc Szálasi, thus creating the impression, if only by omission, that they were the only groups of significance in Hungarian fascism. By contrast Nagy-Talavera stresses the "Szeged fascists" and the Imrédy faction in particular. His conclusion is that the worst atrocities, the most radical excesses, were committed by Imrédy and his associates before Szálasi came to power and not by Szálasi, as is generally assumed. Szálasi's relatively milder regime is attributable to his more moderate principles. His plans for the Jews and non-Hungarians were less extreme than Imrédy's, and his independence from the Germans was greater. Nagy-Talavera thus takes issue with Macartney's less severe view of Imrédy.

The main merit of Nagy-Talavera's book is the thoroughness with which it marshals all the important facts about Hungarian and Romanian fascism. The result is virtually encyclopedic, and who, after all, demands originality of an encyclopedia? The chief weakness of the other book is not Lackó's fault but his publisher's; it is the English translation.

Though the book purports to be in English, it may in fact be completely comprehensible only to those who understand Hungarian. In this instance the Publishing House of the Hungarian Academy of Sciences did not remain true to its own tradition of scholarly, stylistic, and technical impeccability. It should have done so.

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THOMAS A. MEININGER. *Ignatiev and the Establishment of the Bulgarian Exarchate, 1864-1872: A Study in Personal Diplomacy*. (Logmark Editions.) Madison: State Historical Society of Wisconsin for the Department of History, University of Wisconsin. 1970. Pp. xii, 251. \$3.50.

The establishment of the Bulgarian exarchate involved one of those prolonged negotiations in which the relations of the two peoples directly concerned—the Bulgarians and the Greeks in this case—were greatly complicated by the conflicting interests of the powers—primarily Turkey and Russia, but also Austria-Hungary, France, Britain, and Prussia. At the center of these negotiations was Count N. P. Ignatiev, the Russian ambassador in Constantinople. In the interests of Slavic unity he sought to assist the Bulgarians in obtaining religious autonomy without sacrificing other Russian interests: the integrity of the Orthodox Church, and especially the patriarchate of Constantinople of which the Bulgarian bishoprics formed a part; and the maintenance of Russian influence in Constantinople in competition with that of other European powers who sought to support the empire against Russian pressures. The Bulgarians got their autonomous church, thanks in considerable measure to Ignatiev's efforts, but not without a schism with the patriarchate of Constantinople; and Ignatiev's Pan-Slav excesses led to his early retirement a few years later.

Meininger provides a careful and lively account of these negotiations on the basis primarily of Russian and Bulgarian published sources. A fuller account would have required the use of Turkish and Greek sources as well, but by approaching the subject from the perspective of the Russian ambassador the author has been able to provide a reasonably balanced

account using Slavic and Western languages. Ignatiev was the intermediary who negotiated with and reported on all the parties to the dispute, and his dispatches were comprehensive if not always accurate.

Meininger's account retains some of the characteristics of a Ph.D. dissertation, and a few minor stylistic errors remain uncorrected. This volume is published, it should be noted, by Logmark Editions, "designed to make specialized scholarly material available at a minimum cost." Other publishers would do well to imitate this sensible initiative, which offers a serviceable book without the expensive elegance that more often than not is unnecessary for such contributions to scholarship.

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CONSTANTIN C. GIURESCU. *Istoricul Podgoriei Odobeștilor: Din cele mai vechi timpuri pînă la 1918* [The History of the Vineyards of Odobești: From Earliest Times to 1918]. (Academia Republicii Socialiste România, Institutul de Istorie "N. Iorga." Biblioteca Istorică, Number 19.) [Bucharest:] the Academia. 1969. Pp. 550. Lei 32.

This newest monograph from one of Romania's most eminent and prolific historians is the first major study devoted to Romanian wine-growing. The vineyards of Odobești, located in southern Moldavia, were evidently chosen because they are among the oldest and most important in Romania. But Professor Giurescu has given us much more than a history of viticulture. Although he describes the process of wine production in all its complexities, he is just as concerned with general questions of social development.

Based largely upon published and unpublished sources and written in a clear, flowing style, the narrative traces the development of wine-growing in Odobești from its beginnings in the sixteenth century to 1918. In addition to describing the growing and processing of the grapes Professor Giurescu analyzes the forms of economic and political organization that gradually developed in the region. His account of the founding and growth of the market town of Odobești is an important contribution to the history of Romanian cities, and his description of the system of land-

holding and the penetration of capital into the vineyards elucidates a number of problems connected with the status of the free peasantry and its ultimate decline. The sections dealing with the various kinds of taxes imposed upon vineyards, the commerce in wines both at home and abroad, the role of the church and the school in the spiritual and cultural life of the peasantry, and demographic changes throughout the long period under consideration are substantial contributions to Romanian social history.

A major work on the history of Romanian viticulture, this monograph serves two other important purposes as well. It provides the kind of solid information and analysis that are necessary if more general scholarly treatises on Romanian agriculture are to be written, and it stands as a model of what a work of local history should be. An annex of documents comprising nearly half the volume and a detailed index enhance the book's usefulness to scholars.

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MAGDA ÁDÁM. *Magyarország és a Kisantant a Harmincas Években* [Hungary and the Little Entente during the 1930s]. Budapest: Akadémiai Kiadó. 1968. Pp. 389. 72 Ft.

The cyclical relations between the Little Entente and Hungary during the interwar years are the subject of this volume. The Little Entente states, according to the author, had only one common aim—to prevent Hungary's one-sided revision of the Peace Treaty of Trianon. This, however, made the alliance unstable. Czechoslovakia, Romania, and Yugoslavia often had conflicting national interests, and they were unable to coordinate their overall foreign policies toward the great powers of Europe.

Ádám does not provide for a startlingly new interpretation of the twists and turns of Hungarian diplomacy in these years. Hungary's foreign policies have already been explored by others, most notably by C. A. Macartney. But while the latter's writings are often apologetic on behalf of the Horthy regime, Ádám's work is strong in condemning the actions of the Hungarian ruling circles. One of her most serious charges, substantiated by

documents from Horthy's recently published papers, is that Hungary coordinated her foreign policies with Nazi Germany as early as 1937. This assertion will undoubtedly be closely scrutinized by historians since it contradicts Macartney's view that Hungary was a "reluctant ally" of Nazi Germany, trying until the very last moment to avoid open commitment to the Fascist powers.

Ádám's major problem is related to the documentation of her work; she uses some very dubious Western sources in order to buttress her arguments. Furthermore, her dogmatism prevents her explanation of the motives of important Hungarian and Little Entente diplomats from being very effective. On the other hand she makes a determined effort at correcting previously oversimplified views of Hungary's diplomatic relations that have been presented by the apologists of the Horthy regime and by doctrinaire Hungarian historians in the 1950s. She has largely succeeded, and this will make her book worthy of the attention of scholars.

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PETER BROCK and H. GORDON SKILLING, editors. *The Czech Renaissance of the Nineteenth Century: Essays Presented to Otakar Odložilík in Honour of His Seventieth Birthday*. [Toronto:] University of Toronto Press. 1970. Pp. vii, 345. \$10.00.

Otakar Odložilík is probably the most distinguished living Czech historian, and the excellent selective bibliography of his publications from 1923 to 1969 included in this *Festschrift* shows the wide range of his interests. He has concentrated his attention on the Hussite and Reformation periods in Czech history, the rise of the modern Czech nation, and the birth of the Czechoslovak state. Odložilík taught history at Charles University in Prague. In 1948 he moved to the United States, where he became professor of Central European history at Columbia University and later at the University of Pennsylvania.

This volume by seventeen Canadian, American, Czech, and British scholars—mostly historians—testifies to the high regard in which Odložilík is held by scholars and, judging by

the value of the individual contributions, constitutes an impressive tribute. The editing is so well done that this excellent symposium has a high degree of smoothness, without overlapping and duplication, and none of the unevenness in quality that usually marks collaborative studies. The unity of the volume derives from the decision of the editors to devote the examination to a single theme: the history of the Czech national emancipation. Thus from the outset the book gives promise that it will escape some of the weaknesses that often beset *Festschriften*.

Certainly the formation of modern small nations has been among the most important problems for historians of Europe. Whether the formation of these nations is considered a phenomenon of moral grandeur, a result of the impact of ideas, and the outcome of the efforts of the few, or a part of a social process undermining an old, stagnant society and paving the way for the emergence of modern nations, few question its essential importance. In fact the treatment of the Czech revival is sensitively topical, since both Czech and Western historians since 1948 have not brought out any overall study on the major problems of the national awakening, except for some factual surveys and biographical accounts. This is the more surprising in view of the fact that Czech Marxist historians have cast away some of the traditional notions, such as the idealistic thesis of the threatened extinction of the Czech people who were almost miraculously saved by the efforts of a few patriots in the early decades of the nineteenth century.

A great majority of the essays in this volume reflect an integrated, chronological approach to the theme. They cover some of the more important aspects of the Czech renaissance in its three main periods: from the era of "predominantly scholarly interest," through the period of national agitation between the 1820s and 1840s, to the time of the mass national movement around and after 1848 (Miroslav Hroch, p. 34). Most of the essays center on the middle decades of the nineteenth century. The opening three brief contributions—William E. Harkins on the periodization of Czech literary history, Robert Auty on the changing assessment of the role of Josef Dobrovský in the Czech historiography, and Milada Součková on the memory of literary tradition—treat

their topics adequately. In a class by itself is Miroslav Hroch's valuable analysis, "The Social Composition of the Czech Patriots in Bohemia, 1827–1848." The results of his findings are twofold. First, at the outset the clergy formed the largest patriotic group, but in the 1840s their proportion decreased until it almost equaled that of the secular intelligentsia; the number of small merchants and artisans lagged behind that of the intelligentsia. Second, it was not the countryside but the towns that formed the human reservoir for the awakening; thus it was an urban phenomenon.

Stanley B. Kimball lucidly examines the first thirty years of the Czech Literary Foundation. Peter Brock sketches the impact of the Czech revival on the Lusatian Serbs. Joseph Zacek evaluates imperial censorship in the case of the eminent historian František Palacký. Barbara Kohák Simmel gives a pertinent discussion of the role of Karel Havlíček and the Czech press in the era before 1848. The essays by Thomas G. Pešek, John Ericson, Josef Polišenský, Frederick G. Heymann, and Thomas D. Marzik survey the Czechoslovak question, the work of the preparatory committee of the Slav Congress in 1848, the image of the United States in Bohemia, Hussite historiography, and T. G. Masaryk's national background. Francis L. Loewenheim's essay gives an account of Czech-German relations before and during the 1848 revolution, with special reference to the German liberal weekly, *Die Grenzboten*. "The Czechs and the Imperial Parliament in 1848–1849," by Stanley Z. Pech, traces the history of the political hopes of Czech parliamentarians. The remaining two searching examinations cover the years following 1879. H. Gordon Skilling surveys Czech policies up to 1893, and Stanley B. Winters thoughtfully reviews the activities of the Young Czech party, the largest Czech political movement in the years from 1891 to 1901. An adequate index concludes this balanced, scholarly, well-arranged, and attractively designed publication. Useful bibliographical notes by the authors further enhance its value.

It is a pity, however, that the editors did not deem it necessary to include a general selective bibliography on the topic. Specialists on this subject may take exception to certain omissions. Unfortunately there is no coverage of

the economic developments, although some of Odložilík's students have made significant contributions to the economic history of the Czech lands. It is also regrettable that there is no discussion of evidence that the national revival was the natural outgrowth of the complex social process that had accompanied the rise of modern capitalism. The inclusion of the penetrating essay on the Czech renaissance by Miroslav Hroch (*Naše živá i mrtvá minulost* [Our Living and Dead Past] [1968]) from the Czech symposium would have helped to do justice to this widely held thesis. Perhaps an introductory essay should have been added to give scope and depth to the main themes, which often tend to be lost in a welter of details.

In spite of these omissions the study is a proof that a *Festschrift* may be written on a high level of scholarship and offer unity of subject. The research and mass of facts of the essays make the volume a point of departure for English-speaking students seeking information on the subject.

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WACŁAW JĘDRZEJEWICZ, editor. *Diplomat in Paris, 1936-1939: Papers and Memoirs of Juliusz Łukasiewicz, Ambassador of Poland*. New York: Columbia University Press. 1970. Pp. xxvi, 408. \$12.50.

M. K. DZIEWANOWSKI. *Joseph Piłsudski: A European Federalist, 1918-1922*. Stanford: Hoover Institution Press, Stanford University. 1969. Pp. xvi, 379. \$8.70.

THADDEUS V. GROMADA, editor. *Essays on Poland's Foreign Policy, 1918-1939*. New York: Józef Piłsudski Institute of America. 1970. Pp. 71.

Students of East European history are well aware of the importance of the Józef Piłsudski Institute of America for research in modern Polish history. The institute maintains archives related to the political and diplomatic life of Poland between the world wars, and its personnel continually encourage scholarship in this area. Each of the three books reviewed here is, in part, the result of the institute's varied efforts.

Wacław Jędrzejewicz, a former director of the institute, is already well known for his publication of Polish diplomatic source ma-

terials, particularly *Poland in the British Parliament* (1946-64) and *Diplomat in Berlin* (1968), the memoirs of Józef Lipski. *Diplomat in Paris*, the memoirs of Juliusz Łukasiewicz, is similar to the latter. Jędrzejewicz has combined published and unpublished memoir fragments, skillfully filling the gaps with over sixty previously unpublished Polish diplomatic documents and an explanatory text. The only flaw in the editing is the lack of any guide to the location of the documents scattered through the book.

Łukasiewicz was typical of many interwar Polish diplomats. In his youth he was, like Piłsudski, a Polish patriot, revolutionary, and socialist. His socialist sympathies caused him to be named ambassador to Russia in 1933 and in 1936 ambassador to the new Popular Front government in France, where he remained until the outbreak of the war. The memoirs, which were written for the most part in the eight months from November 1939 to June 1940, deal with this embassy. France, wracked with internal dissension and self-doubt, wished only to be left alone. Instead of security, her Eastern alliances threatened her with an unwanted war with Germany, yet she could not honorably disengage herself from them. This dilemma was resolved by maintaining the alliances and abandoning the allies. Łukasiewicz's efforts, as ambassador of one of those allies, to force France to honor her obligations in East Europe failed. Promises of diplomatic support at Munich were forgotten; promises of financial credits for armaments were unfulfilled. Finally, after the invasion of Poland, the French land and air offensives promised in the treaty of 1921 never materialized. Despite all this, the memoirs are sad, not bitter. Together with the documents included in the text they are a valuable contribution to the growing store of material on the origins of World War II.

The archives of the Józef Piłsudski Institute formed the basis for M. K. Dziewanowski's study of Piłsudski as a European federalist. Piłsudski's federalism is described as an instinctive attempt to solve Poland's post-World War I strategic problem within the framework of the medieval Jagellonian Polish-Lithuanian confederation. Such a solution came naturally to Piłsudski, who was born into the Polonized

Lithuanian gentry living in the border zone between Polish and Russian culture. Taking advantage of the fluid political situation that developed in Eastern Europe due to Germany's defeat and the Russian Revolution, Pilsudski sought to convince the Lithuanians and the Belorussians to re-create the grand duchy of Lithuania and again federate with Poland. This attempt failed because of the chauvinism of the Lithuanians, the underdeveloped nationalism of the Belorussians, and the opposition of Polish nationalists such as Roman Dmowski, who wanted a national rather than a federal Polish state. Pilsudski's federalism also extended to the Ukraine, which he hoped to see independent and loosely tied to Poland. The result was the romantic but disastrous Ukrainian campaign of 1920, which failed because of the immaturity of Ukrainian nationalism. As a result of these failures the territorial structure of Eastern Europe crystalized on national lines with Pilsudski's federalist dreams unfulfilled.

Dziewanowski's interpretation will not be new to anyone who has read at all deeply in this period. He adds new detail, however, and gives his thesis the benefit of a coherent presentation. The book is unfortunately marred by poor editing. There are numerous inconsistencies in the spelling of names (Hadziacz/Hadiach, Vinnichenko/Vynnychenko), while the skimpy index limits the book's reference value. It is also surprising that despite the author's exhaustive research in most areas, he has ignored League of Nations sources, even though Wilno (Vilna) was a major League issue. Outside of this, however, Dziewanowski has provided a useful guide to a complex issue.

The fiftieth anniversary of Poland's independence and the twenty-fifth birthday of the Piłsudski Institute were celebrated in New York in November 1968 with an academic conference. Papers presented there on Polish interwar diplomacy were later published in Gromada's small book. The diplomacy surrounding Poland's struggle for independence receives most attention. A. G. Kuczyński reviews wartime diplomacy, M. K. Dziewanowski treats aspects of Pilsudski's federalist policy in Eastern Europe, and Piotr Wandycz discusses the long-term ramifications of the Treaty of Riga. Essays by Christoph Kimmich

and Roman Dębicki survey the Polish-German border question in the 1920s and the deterioration of Poland's alliance with France. The final essay is by the editor, Thaddeus Gromada, and deals with the role of the Slovaks in the failure of Józef Beck's "Third Europe" scheme in 1938. Like most *Festschriften* this collection suffers from the lack of a central theme and an unevenness in the quality of the essays, most of which are quite brief. Its main value lies in giving the reader a quick taste of some of the current scholarship on Polish interwar diplomatic history.

WILLIAM J. WOOLLEY
Ripon College

NICHOLAS BETHELL. *Gomułka: His Poland, His Communism*. New York: Holt, Rinehart and Winston. 1969. Pp. 296. \$5.95.

The revolt of the Polish Decembrists in 1970 for all practical purposes ended the political career of Władysław Gomułka, who had ruled Poland for some fourteen years, since October 1956. Apart from Tito, Gomułka was the only European leader who successfully challenged Stalin and survived to win back political power. In a way Gomułka's achievement was more remarkable than Tito's, since Gomułka was effectively in Stalin's power. On the other hand the Polish leader was careful enough never to go as far in his defiance of Moscow as the Yugoslav chieftain.

In the first chapter of his book Bethell stresses that "no one is yet in a position to write a complete biography of this man" since most of the essential sources for such an undertaking are still denied to historians. Another difficulty encountered by historians is Gomułka's dislike of personal publicity. He regards it as un-Marxist and tries to "protect himself as a faceless symbol of socialist Poland." And indeed Bethell has written a book that is more about Poland under communism than a typical biographical study. The work is based primarily upon published sources. The author deals in detail with Gomułka's eclipse in 1948-53, his "finest hour"—the autumn of 1956, and with what he calls his "darkest year"—1968—when the youth revolt in Poland followed by the Soviet invasion of Czechoslovakia shook the Polish regime to the core.

Bethell's explanation of Gomulka's stern behavior during the 1960s is to be found in the pervading danger of a Russo-German rapprochement at Poland's expense. The danger was dramatized by Adzu Bey's mission to Bonn in 1964, a mission that alarmed the Polish leader as a possible first step toward German reunification. Gomulka's role in the overthrow of Khrushchev is not yet fully known, but there is enough evidence to say that the Polish party leadership exercised whatever limited influence it had at that time on the side of Khrushchev's enemies. Bethell observes that "the whole affair shook the Polish Party boss horribly. He saw it as a confirmation of his worst fears. Poland had had a hair-breadth escape from becoming yet again a matter of Russian-German bargaining. Gomulka had long ago discarded what he thought were the over-liberal ideas of the Polish 'October.' . . . But after the experience of 1964 he began to turn into that epitome of conservatives, the man who is frightened of political change. . . . Gomulka's former flexibility, his lack of Marxist dogma, became hardened by his obsession with preserving the integrity of the Polish state, both her existence as a separate political entity and her western frontiers. . . . Gomulka's greatest complaint against his political opponents is that they take Poland's existence for granted, that they act as if Poland were a self-sufficient island. He sees such men as totally unrealistic and does not hesitate to accuse them of being ready to sacrifice Poland's vital interest for the sake of such luxuries as freedom of expression" (pp. 244-45). This is, according to Bethell, the key to the behavior of the Polish leader during the last decade, but especially since 1964.

The author shows a sympathetic understanding of Gomulka's difficulties without subscribing to his views or excusing some of his obnoxious methods. The author nevertheless stresses that "Gomulka is no anti-semitic" and that he "was dragged along by events, by a situation he did not control" (p. 264). He tried to limit the so-called anti-Zionist campaign; his speech of March 14, 1928, had this very purpose.

The downfall of Gomulka in December of 1970 has been followed by a series of revelations concerning his style of handling the party

machine and the details of his life. Meanwhile, Hoffman & Campe of Hamburg has published a volume of reminiscences by Gomulka's interpreter, Erwin Weit—*Ost-Block Intern: 13 Jahre Dolmetscher für polnische Partei- und Staatsführung* (1970). All this makes Bethell's book somewhat obsolete. It nevertheless remains a valuable attempt to sketch the broad background of Gomulka's activities, especially during the period 1956-70.

M. K. DZIEWANOWSKI
Boston University

ALEXANDER V. BERKIS. *The History of the Duchy of Courland (1561-1795)*. Towson, Md.: Paul M. Harrod Company. 1969. Pp. viii, 336. \$12.50.

This is the first full scholarly history of the Duchy of Courland in English and therefore welcome news for all who are interested in Northeastern and Central European history. It is a narrative history organized chronologically. Berkis has not attempted to present an argumentative account of the duchy but rather to synthesize the existing points of view. The majority of historiographical contributions on the duchy are by German scholars, although Latvians have also contributed considerably. Berkis, a Latvian, has avoided the quarrelsomeness that frequently accompanies the writing on this topic. For example, the author seems to be the first Latvian who has ever found anything positive to say about serfdom in Courland. On the other hand, he has also striven to show the contributions to the welfare, prosperity, and life in general of the native population that are frequently neglected in accounts by German scholars.

The study begins in 1561 when the first duke of Courland, Gotthard Kettler, arranged for Courland to become a fief of Poland. It ends with the annexation of the duchy to the Russian Empire in 1795. The author devotes special attention to the reign of Duke James (known in German and Latvian accounts as Jacob), 1638-82. In this period Courland enjoyed a prosperity and fame that attracted the attention of many European courts. The duchy may have been the most important principality of its size in Europe during the reign of James, and it rivaled Prussia. The success of the duke rested not only on his mercantilist policies, the establishment of overseas colonies,

and his fleet, but also on his prudent fiscal policy and administrative abilities. Internal difficulties with nobility and international ones with Sweden and England, however, proved too much for the energetic duke, and according to the author the decline of the duchy set in before the death of the duke. The duchy, in the author's view, enjoyed another brief period of international fame under Duke Biron who, through his connection with Anne of Courland, came to play a significant role at the court of St. Petersburg. As opposed to the traditional accounts of the duchy, which have tended to be mostly political, Berkis devotes some attention to social life, especially that of the peasantry, and he even has a chapter on religious and intellectual life in Courland.

The work is fully footnoted and contains a lengthy bibliography. Neither in the footnotes nor in the bibliography is there a single mention of the contributions made by Walter Kirchner, perhaps a minor but an inexplicable failing. The work could have profited stylistically from a more exacting editor.

ANDREW EZERGAILIS
Ithaca College

CARSTEN GOEHRKE. *Die Wüstungen in der Moskauer Rus': Studien zur Siedlungs-, Bevölkerungs- und Sozialgeschichte*. (Quellen und Studien zur Geschichte des östlichen Europa, Number 1.) Wiesbaden: Franz Steiner Verlag GmbH. 1968. Pp. xi, 357.

This monograph, the first on its subject, auspiciously begins a series dedicated to medieval and early modern East European history and is edited by a scholar distinguished for contributions to that field and published by a press outstanding for its volumes in geography. Goehrke's major purpose is the analysis of wastelands in Muscovite Russia both from the viewpoint of historical geography, that is, their role in the changing landscape and the patterns of settlement, and from the viewpoint of social and economic history, that is, their function as indicia of changes in population and the economic and agricultural structure. Other purposes include the clarification of relevant terminology in the sources, the identification of the periods in which wastelands increased, and an explanation of reasons.

After sketches of German and Russian re-

search, the author describes various sources and imaginatively suggests circumstances and motives affecting the trustworthiness of some. A detailed chapter on terminology with explanations of shifting meanings is followed by five on the periods in which wastelands developed and by four on reasons for the most important of the periods. Among the reasons for wastelands in the mid-fourteenth century and the first three decades of the fifteenth century, the author specifies both the plague and the concentration of the population in fewer settlements and speculates whether the West's economic crisis exercised any influence. An increase in Church holdings resulted: laymen rid themselves of land yielding little or no income and helped save their souls (p. 67). Although in the "extended vicinity" of Moscow the number of settlements declined by seventy-five to eighty per cent between the sixteenth and nineteenth centuries, and in North Russia and the middle Volga area a substantial reduction occurred in the seventeenth and eighteenth centuries, Goehrke finds that the nadir occurred between the mid-sixteenth century and the second quarter of the seventeenth century. Reasons range from the less significant—the oprichnina (1565–72) in which, among other things, many peasants were killed or died of famine or epidemics—to deaths in the Livonian War, and to the most significant, the exodus of peasants to escape debts and taxes mounting almost uninterruptedly from the mid-sixteenth century—all leading to economic disaster (p. 165). The exodus was to the northern coastal region, to Viatka, Perm, and the Urals, occasionally to foreign countries, also to Siberia, and most often to the black soil region of the middle and lower Volga, the last two made possible by contemporary Russian territorial expansion. Later reasons also include the Time of Troubles and the devastation of parts of Russia by war. Social mobility too may have had a negative effect, but the weather, being generally favorable, rarely did. Villages, whether in lay or in clerical hands, generally grew larger, often swallowing numerous small settlements that disappeared (p. 151). Service estates (*pomest'ia*) were undermined, while patrimonial estates (*votchiny*) were often strengthened. Reasons for the consolidation of settlements (*Siedlungsbul-*

lung) included the wish of landholders to use land more rationally and the imposition of services upon them. Important effects were greater control over peasants, a reduction in individual, far-flung fields, a tendency toward an equalization of the peasantry's economic levels, and ultimately, the communal system.

The shortage of published sources about specific farms and fields and his lack of access to Soviet archives may have hindered Goehrke from executing the complete historical geographical analysis he desires, yet he is remarkably thorough. Moreover, he states his doubts clearly, for example, about percentages of fields that became wastelands (p. 123) and risks reasonable decisions despite minor doubts, for example, in his treatment of all *pustoshi* and *selishcha* as wastelands (p. 97). He discusses some debatable matters insufficiently, as the development of service estates under Vasilii II (p. 79) or the later growth of an agricultural proletariat (p. 213). Comparisons add clarity, for example, that, unlike West European cities, those in Muscovy profited little from agrarian troubles because grain prices rose (p. 259).

Goehrke's bibliography is extensive, flawed by unexplained omissions within published series listed. Were some volumes unavailable? A detailed index and three cleanly drawn maps add much.

OSWALD P. BACKUS III
University of Kansas

ALEXANDER VUCINICH. *Science in Russian Culture, 1861-1917*. Stanford: Stanford University Press. 1970. Pp. xv, 575. \$18.50.

With this work Professor Alexander Vucinich of the University of Texas presents the second in his three-volume study of science in Russian culture, an achievement of major significance. The first book, published in 1963, covered Russian science from its origins to 1860; this second volume covers the period 1861 to 1917. Presumably the final installment will cover Soviet science from the Revolution to the present.

As was the case with the first volume, the real contribution of Vucinich's work lies in its illustration to non-Russian language readers of the richness, venerability, and achievement

of Russian scientific thought. With the help of Vucinich's volumes intellectual historians whose main attentions are devoted to Western Europe and America will be able to incorporate material on the history of science in Russia into their lecture courses and research. Science in Russia began not after the Revolutions of 1917 but much earlier; Vucinich writes that by the 1860s science ceased to be a secondary intellectual force in Russia and instead emerged as one of the major influences upon the cultural history of that nation. Already by that time a number of Russian scientists had made contributions to knowledge that had won—or soon would win—international recognition. Some readers may see this emphasis on pre-Revolutionary Russian science as a de-emphasis of the Soviet achievement, but it seems apparent from Vucinich's approach that such a goal is not his intention. The final result of his research will, in all likelihood, be a greater appreciation of Russian and Soviet scientific thought throughout its entire history from the early eighteenth century to the present time. Such a result will be a valuable counterbalance to the attention given by many Western historians to mystical or irrational elements in Russian culture.

The achievement of Vucinich's work also partially explains one of its weaknesses. His emphasis is much more on the recognition of Russian scientific contributions and the interaction of science with its social milieu than it is on the conceptual problems in scientific thought itself. The major part of the work consists of rather literal summaries and descriptions of the works of many Russian natural historians, sociologists, and philosophers. Partly because of the encyclopedic nature of the study and partly because of an inadequately critical intellectual stance, the genuinely difficult problems of the analysis of nature are very rarely faced. The scholar seeking, for example, discussions of the intellectual origins and analytical problems involved in Butleróv's theory of chemical structure or of Mendeleev's periodic table—two of the most significant events of the period—will not find them here. He will find, however, a persuasive and felicitously written account of these men's achievements, their views on general topics concerning science and society, and a state-

ment of the overall philosophic significance of their work.

Alexander Vucinich's research on the history of Russian science is highly valuable both on its own merits and for its portrayal of a host of research topics for subsequent scholars. As a reviewer of the earlier volume commented, "There are the seeds of at least a dozen good dissertations in Vucinich's work."

LOREN R. GRAHAM
Columbia University

JOHN P. MCKAY. *Pioneers for Profit: Foreign Entrepreneurship and Russian Industrialization, 1885-1913*. Chicago: University of Chicago Press, 1970. Pp. xiii, 442. \$11.50.

In many ways, according to McKay, the most original aspect of the Russian scheme for economic development in the 1890s and after was its emphasis on direct foreign investment. Von Laue in his study of Witte's role in Russian industrialization asserts that Witte bet on a massive response from private producers. McKay carries the analysis farther and argues that Witte bet particularly on foreign private entrepreneurship with its superior technical knowledge and financial resources. This became the distinctive Russian addition to the standard nineteenth-century mix of railroads, tariff protection, and state patronage as the means to industrialization.

The extent, nature, and effects of direct foreign investment on Russia's economy are the larger questions with which McKay's study is concerned. It is also concerned with the larger problem of what contribution foreigners from economically advanced countries can play in poor countries. To answer these questions, however, one must find out first what foreigners did in Russia, and most of this study is given over to that subject.

Despite much diversity in time, place, and origin, McKay discerns a typical pattern of foreign investment: 1893-1900 marked the period of greatest inflow of direct investment capital, usually French or Belgian and directed into mining and metallurgy, particularly in southern Russia. It is on the parts of this pattern that he concentrates: the types of foreign entrepreneurs, their investment motivation and strategy, the growth impact on the Russian economy, employment practices of Russians as against foreigners, and changes in the

mobilization of capital. Several case studies of particular companies complete this work.

The author concludes that foreign entrepreneurs provided and mobilized the missing capital needed for industrialization, that they successfully implanted advanced techniques in several key industries, and that they infused a missing dynamism and growth outlook into Russia. These successes became possible because of Russia's independence as a nation. There was no imbalance of power in favor of the foreigners; on the contrary, it was in favor of the state, which took care not to abuse its preponderance. This Russian experience indicates that foreign entrepreneurship, within limits, can be beneficial to economically backward countries.

McKay develops in depth an aspect of Russian industrialization that Von Laue scarcely explored, and his conclusions spotlight an area of success in Witte's program, a program that as a whole Von Laue sees as a failure. This carefully researched study with its balanced and judicious analyses is a "must" for students of modern Russian economic history, of the kind on which sound generalizations can stand.

RAYMOND H. FISHER
University of California,
Los Angeles

EZRA MENDELSON. *Class Struggle in the Pale: The Formative Years of the Jewish Workers' Movement in Tsarist Russia*. New York: Cambridge University Press, 1970. Pp. xi, 180. \$8.50.

In this excellent study of Jewish workers in the Russian Pale the author claims to "focus on the activities of thousands of workers attracted to the labor movement" and on the expectations they had of that movement. He is concerned principally, but not exclusively, with the movement fostered by the Russian Bund—the Jewish Marxist party—but tells us he is "emphatically not concerned" with the development of Bund ideology. Moreover, he seems very little interested in ideology at all, so that even though he is dealing with thousands of workers sharing a common language and religion, foreign in the land in which they lived, the author tells us nothing of the basic assumptions of Pale Judaism nor of the ideologies that vied with it for allegiance of Jewish workers. Thus the author's illuminating ac-

counts of the organization of the workers, their relationships with Jewish intellectuals, with their employers, with non-Jewish workers, and with the state are not sufficiently fitted into the context of the situation and ideas in which they were found. As a consequence, in spite of the validity of the author's historical description, it seems at times that he is dealing with his subject in so abstract a way that the reader may lose sight of the meaning, to the workers, of the organizations they joined or built. No doubt the author attains a commendable degree of objectivity by his abstraction. His history of Jewish workers in Pale also acquires a degree of universality thereby, but the author, quite consciously it seems, neglects to such a degree the context of mood and idea with which he is so well qualified to deal that he might at times be writing about Anglican workers in Surrey.

In his conclusion 2 the author claims that Jewish workers in the Pale were organized before Russian labor in the interior, because Jewish workers were artisans with a history of guild organization, whereas Russian factory workers were peasant as well as proletarian. But might not ideology have contributed something to the phenomenon of earlier organization? The author does not ask that question.

Such questions, however, are my own personal predilection. And in conclusion 1, in which the author deals with the meaning of the labor movement to the Jewish workers when he tells how the movement transformed the lives of some of them, how it made "new Jewish men" out of former Orthodox artisans, his work becomes vibrant.

Drawing skillfully together information from Yiddish, Hebrew, Russian, and German sources, this book is of serious interest to all those concerned with labor history, Russian history, Jewish history, and the history of socialism.

FREDERICK L. KAPLAN
Michigan State University

A. IA. AVREKH. *Stolypin i Tret'ia Duma* [Stolypin and the Third Duma]. (Akademiia Nauk SSSR, Institut Istorii.) Moscow: Izdatel'stvo "Nauka." 1968. Pp. 519.

The politics of the Duma monarchy have attracted more attention among English-speaking

historians than they have in the Soviet Union. In the past few years, however, Aron Iakovlevich Avrekh has been conducting a strenuous one-man campaign to redress this balance. In addition to some general writing he has provided essays on the military question, the Kholm affair, and on labor legislation in the Duma monarchy. In his first major book he attempts to "definitively decide" the central question of the Duma era: "whose was the victory—the revolution or the counter-revolution?" His conclusion is predictably in favor of the revolution, but his approach to the problem is in many respects unexpected and even revisionist (as a historian, not a Marxist). Avrekh has chosen to focus on the closing years of the Third Duma (1910–12) and to omit the agrarian question and even the mainstream of Stolypin's career. Instead he has loosely linked a number of essays on other issues from these years: the national question, with special reference to Finland and city government in Kholm; the labor question; and "the downfall of Stolypinist Bonapartism," including the assassination of the premier and the disarray in Duma politics.

In the introduction to this book, as in his other writings, Avrekh pays tribute to the standard Leninist formulas concerning the period: Stolypin was a "Bonapartist," he depended on the mobilization of two alternative Duma majorities (the Octobrists and the right or the Octobrists and the Kadets), his agrarian policy favored the Prussian over the American model. The best sections of the book, however, leave these verities far behind, advancing some ideas that should interest Communist and non-Communist historians alike. The most innovative interpretive work appears in the section on the "national question." Soviet usage generally restricts this phrase to the minority nationalities, but Avrekh emphasizes instead the question of Great Russian nationalism and the preservation of the Empire. Contrary to Lenin, Avrekh asserts that this was "the most vital question, to which all the remaining internal political problems, including the agrarian question, were subordinated and dependent." His elaboration of this thesis is uneven. The Union of the Russian People receives scant attention, while some other nationalist organs are discussed in detail, but Avrekh's thinking is informative and suggestive. Peter Struve's writings—rarely mentioned in the USSR—seem to have been

influential in stimulating Avrekh's analysis, even though the renegade Marxist writer is always referred to with suitable scorn. Struve is shown to have pressed on the liberals the challenging idea that nationalism was crucial in the control of the Russian polity in any form, and that the liberals should have recognized frankly that they had no future if they could not become identified with militant nationalism. One hopes that Avrekh will extend his studies a few years and show how this tactic, not very successful around 1910, could be considered the key to the February Revolution. In the course of his discussion on this point Avrekh strains somewhat to show that the liberals became open anti-Semites, noting in passing that attitudes on the Jewish question can be considered a "barometer" of Russian chauvinism. This is not a bad idea and an interesting one to hear from a Soviet scholar in these days.

Avrekh's discussion of the labor question, based on substantial archival study, also offers some novelties in Soviet historical thinking. Vladimir Kokovtsev, the finance minister, emerges as an ambitious advocate of welfare state policies, while the conservative ideologist Tikhomirov is seen as attempting to persuade Stolypin of the need for state-controlled labor unions (more or less like later Soviet unions). In other words, the highest bureaucrats appear as modernizing *étatists* rather than as representatives of the bourgeoisie. True, Avrekh shows convincingly that the industrialists' lobby was able to force major concessions in the framing of the labor legislation of the Third Duma, but the resistance of the bureaucrats to capitalist interests is as significant as the compromises that they made.

A substantial section of the book is devoted to the mystery of the Stolypin murder, which was reopened in 1966 by B. Iu. Maiskii who discovered in Kiev some new records connected with the trial of the assassin Bogrov. On the basis of a detailed and rather involuted examination of the evidence, Avrekh concludes that Maiskii is wrong in believing that Bogrov was a dedicated revolutionary (his argument is persuasive here) and he suggests that the real manager of the plot was the *okhrana* officer Kurlov, who was gambling on the guess that the tsar would be glad to see Stolypin

gone if decent appearances could be maintained. Kurlov's role cannot be dismissed, but the weakness of Avrekh's handling of his interpretation is the vague, scarcely researched treatment that he gives to the whole idea of a conspiracy against Stolypin that involved the tsar and the *verkhii* (generally unidentified courtiers, not including Rasputin, whom Avrekh explicitly considers unimportant). This treatment is as unsatisfactory as the treatment of Stolypin as a nonentity whose personal political career is not worth extended study.

It would require a much longer review to call attention to all the other points in this work that deserve serious reading—archival revelations on the drafting of the new Finnish law or the rather spineless dealings of the Kadets with their comparatively radical youth auxiliary, for example. Avrekh tends to overwrite, filling too many pages with direct quotations from such sources as the Duma minutes, but this tendency is an asset to his foreign readers when he is dealing with a number of previously unused archives. As a whole the book may be more profitably read as a collection of related essays than as a general history of the Third Duma or the fate of revolution in that period. But it is probably the most interesting Soviet book on twentieth-century Russian history to appear in many years.

ROBERT H. MCNEAL

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Amherst

CARL B. TURNER. *An Analysis of Soviet Views on John Maynard Keynes*. Durham: Duke University Press. 1969. Pp. vii, 183. \$6.50.

IA. E. CHADAEV. *Ekonomika SSSR v period velikoi otechestvennoi voyny (1941–1945 gg.)* [The Economy of the USSR during the Great Patriotic War (1941–1945)]. Moscow: Izdatel'stvo "Mysl'" 1965. Pp. 388.

Professor Turner describes the changes in the Soviet attitude toward Keynes and in the Soviet interpretation of Keynesian economics as the Soviet political and ideological climate has changed over the past forty years. Keynes was early accorded considerable attention in the Soviet Union, well before the appearance of the *General Theory of Employment, Interest and Money*, largely because his analysis of the peace settlement after the First World War in

The Economic Consequences of the Peace corroborated Soviet views on contradictions among the capitalist powers. Lenin mentioned his analysis approvingly, and *Economic Consequences* was translated into Russian, as were several other of his works in the period before the Second World War.

Soviet economists and leaders were apparently slow to appreciate the theoretical revolution advanced in the *General Theory* when it appeared in 1936 and had little understanding of its implications for the future of the capitalist system. The central role that its ideas came to play in Western economic literature finally made the Russians aware of its significance, and a Russian translation was published in 1948. The work was presented objectively enough in the translation without cuts or distortions, though it was prefaced with an introduction intended to refute the analysis. But the period after 1948 was an inauspicious time for any Western idea to receive objective interpretation in the Soviet Union, and for the next several years the neutrality of this first presentation was succeeded by vitriolic denunciation. Soviet commentary in these years came to consist almost exclusively of defamation of Keynes and obscurantist obfuscation of Keynesian analysis, the main purpose of which was to warn Soviet readers away from the unwelcome conclusion that the expected collapse of capitalism could be staved off indefinitely by proper fiscal policies. After Stalin's death, this line gave way to a more objective interpretation of the theory itself and to a more rational appreciation of the potential of its policy implications for prolonging the existence of capitalist society.

The book is a fairly simple exercise, which traces through copious quotes and paraphrases the changing treatment Keynes received at Soviet hands. It contains a very extensive bibliography of the Soviet commentaries on Keynesian views, particularly on the *General Theory*, which is interesting, incidentally, as a checklist for identifying the Soviet economists who have played the ignoble role of "learned hand-maidens" of Soviet ideology. But despite the book's title, there is not a great deal in this story to analyze, since the Soviet views have tended to be political diatribes rather than economic critiques. All the same, this is an

important and instructive story, useful to have recorded in this fashion. Their encounter with Keynes is a prototypical example of the difficulties Soviet economists have had during much of the Soviet period in interpreting the economic analysis developed in the capitalist world and assessing its relevance or implications for their own policy problems or for their views of the world.

The Chadaev book is a sample of the considerable literature on the World War II economy that has appeared in the USSR since Stalin's death. It is not an especially informative representative of the class, however. There is little data in it that have not appeared elsewhere, and it lacks the kind of revealing detail on important decisions and policies that the memoir literature has contained. What it does very well is to convey the sense of austerity and the stark priorities that diverted everything to the demands of the war, with other needs left to be satisfied with whatever substitutes local ingenuity could manage.

The objectivity and realism of the book can be gauged by two points. First, Chadaev says that "the whole burden of the Second World War lay essentially on the shoulders of the Soviet Union alone." Second, although the book starts out with a discussion of the economic base and the issue of preparedness for the war, Stalin's name is mentioned for the first time on page 58. It appears once again for the last time on page 93. The general theme of the book—that victory in the war was due to the party and government—is no doubt an improvement over the earlier line that everything was due to Stalin, but it still gives remarkably little credit to the Russian people.

ROBERT W. CAMPBELL
Indiana University

NEAR EAST

G. E. VON GRUNEBaum, editor. *Logic in Classical Islamic Culture*. (Giorgio Levi Della Vida Conferences. Published under the auspices of the Near Eastern Center, University of California, Los Angeles.) Wiesbaden: Otto Harrassowitz. 1970. Pp. 142. DM 56.

This work is a collection of five papers presented at a conference held at the University of California at Los Angeles in 1967.

In the first paper, "Logic and Law in Classical Islam," Robert Brunschvig deals with jurisprudence and legal argumentation, making the point that the logic of the lawyers was not the technical logic of the philosophers in that "the logical foundation they [that is, the jurists] strove to assure . . . did not go so far as the rigor of the syllogism" (p. 18).

Josef van Ess's discussion of "The Logical Structure of Islamic Theology" is particularly valuable. It shows that the dialectics of philosophical theology has close contacts with logic in the technical sense. Specifically, it affords many interesting illustrations of the role of the ideas of Stoic logic in the *kalām*.

In his paper on "Language and Logic in Classical Islam" Muhsin Mahdi deals with Arabic views of the relationship of the informal logic of everyday discourse to the technical logic of the Aristotelian philosophers. His focus is the interesting controversy between Abū Sa'īd al-Sirāfi and Abū Bishr Mattā ibn Yūnus in the early tenth century, to which attention had been drawn earlier by D. S. Margoliouth (in the *Journal of the Royal Asiatic Society*, in 1905). The discussion proceeds without any real contact between the opponents because "throughout the debate, al-Sirāfi understands 'logic' (*mantiq*) as arab philologists had understood it before him" (p. 73)—that is, as the conceptual structure of a specific language—and rejects any dealings with the technical machinery of the philosophical logicians.

In his paper "Poets and Critics in the Third Century A. H.," S. A. Bonebakker deals with the views of the philologists in their study of poetry, focusing on questions of style and poetic device, using this as a basis for a consideration of Arabic literary criticism in the third century. Had not the Arabic logicians themselves misguidedly classed poetics as a branch of logic, I would think this paper to fall outside the scope of the symposium.

In the final paper, "The 'Law Merchant' of the Medieval Islamic World," A. L. Udovitch discusses the evolution of Islamic law as it relates to merchants and mercantile practices. Useful enough in its own right, it is difficult to see what substantive linkage subsists between this paper and the logical theme of the rest, unless we follow the tendency of the author's

one and only reference to logic in his final paragraph and speak of a "logic of practical reality" (p. 130), introducing into scholarly discourse the lamentable precedent of people who speak of a "philosophy" of salesmanship or tourism.

This is a useful collection, some of whose papers informatively illuminate the way in which logic was regarded by substantial sectors of the learned community of medieval Islam (legal theorists, theologians, and philologists). Overall, however, a markedly negative view of logic (that is, technical logic) emerges. This is no fault of the contributors but results from the fact that virtually no one in medieval Islam save some of the philosophers had a good word to say for logic. The organization of this conference may thus be faulted for omitting from this examination of "logic in classical Islamic culture" a consideration of the contribution made by logic within the one significant sector of the intellectual culture of medieval Islam that tended to view it positively—to wit, philosophy.

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JACOB LASSNER. *The Topography of Baghdad in the Early Middle Ages*. Detroit: Wayne State University Press. 1970. Pp. 324. \$12.50.

In the absence of any excavation of early Baghdad a study of that most important city founded in 762 by Caliph Al-Manṣūr has to rely entirely on accounts in descriptive geographies such as Ya'qūbī's *Kitāb al-Buldān* (ca. 891) and Suhrāb's *'Ajā'ib al-aqālīm as-sab'ah* (ca. 925), in histories such as the *History of Baghdad* of Aḥmad b. Abī Tāhir Ṭayfūr (died 893), and in the pseudohistorical works in praise of the characteristics (*khawāṣṣ*) and qualities (*faḍā'il*) of a city.

Though religious-minded, the history (*Tārīkh*) of Baghdad by Khaṭīb al-Baghḍādī (died 1071) is by far the best written source on the topography of that city. Four of the chapters of its introduction describe the Round City, the west and east sides, and the hydrography around the capital. The work relies heavily on older materials, but it is the only one that gives specific references (*isnād*) to the earlier authorities.

Professor Lassner attempts "to differentiate

the chronological layers of the text" and hence to evaluate the growth of Baghdad during the two centuries from its foundation to the tenth century. The new annotated translation in English (pp. 45–118) is most needed, since the early (1904) and nearly unavailable French translation by G. Salmon failed to cover systematically the material then published. In addition Professor Lassner puts historians of urbanism and of medieval Islam in his debt with his most valuable contribution: "The Topographical Growth of an Imperial City," based on his chronological re-evaluation of Khatib's work.

Was Baghdad perhaps intended to unify the widespread regions of the 'Abbassid Caliphate after the sumptuous manner of the Sassanids at al-Madā'in (Ctesiphon) (pp. 136–37)? The new capital developed, contrary to garrison towns (*amṣār*), from the outside in, within the huge Round City. Professor Lassner offers a theoretical reconstruction of this city, consisting of the outer fortifications (two concentric walls separated by an intervallum) accessible from four gates, an inner residential area, a ring with the residences of the caliph's children and government agencies (not provided in E. Herzfeld and F. Sarre, *Archäologische Reise im Euphrat- und Tigris-Gebeit* [1911–20] or in K. A. C. Creswell, *Early Muslim Architecture* [1932–40]), and the inner court with the caliph's residence and mosque (cf. figs. 1 and 2).

Al-Manṣūr became aware of the deficiencies of his Round City through the remarks of the Byzantine ambassador, and he relocated the markets first to the rooms flanking the arcades, then to the ancient suburb of al-Karkh, and returned the guards to the arcades. Still uneasy about his personal security, he built a palace, al-Khuld, to the east outside the city and another, ar-Ruṣāfah, opposite the city on the eastern bank of the Tigris for his son al-Mahdi.

Let us remark that neither Baghdad nor its possible prototypes Dārābjird or Gūr (according to Creswell, but denied by Lassner) could offer its ruler the security of a central citadel looming high within its own walls as in the eighth-century B.C. circular city of Sam'al.

Lassner offers a tentative interpretation of the enlargement of al-Manṣūr's Mosque (pp. 189–95) that differs from that of Herzfeld and Creswell (figs. 10–14). Abundant notes and

indexes make this work an indispensable reference and stimulating reading.

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STANFORD J. SHAW. *The Budget of Ottoman Egypt, 1005–1006/1596–1597*. (Publications in Near and Middle East Studies, Columbia University. Series A, Number 12.) The Hague: Mouton. 1968. Pp. viii, 234. 72 gls.

Professor Shaw has made available in translation another important type of source for understanding the history of Ottoman Egypt—an annual budget. The study begins with a brief outline of the general resources and expenditures that would be included in almost any Ottoman Egyptian budget. Shaw then gives a transcription and an extensively footnoted translation of the budget for 1005–06/1596–97, with photographs of the manuscript at the end of the work. By placing his extremely illuminating notes on the same page as the translation Shaw makes it possible for the reader quickly to get a sense of the breadth and depth of Ottoman rule and financial arrangements. For example, in the first section, concerning revenues, references are made to the expected sources—land taxes and so forth—and also to such specialized items as the repayment of a loan by the *qadi* of Mecca who had needed money to buy supplies in order to provision the city during the pilgrimage. The details in the next section, concerning expenditures, are even more fascinating and run from the payments for officials to the cost of the camels that carry the money to the *qadi* of Medina. The final section, on balance or remainder, indicates that most of the surplus, although not all of it, went to Istanbul.

The transcription, translation, and photographs have been carefully numbered by Shaw so that one may easily move between the three sections. Even with these aids the special *siyakat* administrative script appears to me as unfathomable as ever, and I stand in awe of Professor Shaw's ability to unravel its secrets.

The question of why this particular budget was selected is not satisfactorily answered. Shaw states that it is the earliest complete budget, but is it unique in any other way? Were the

revenues the highest or expenditures lowest? Is there any way we can estimate how that year's balance sent to Istanbul compared with other resources available to the sultan? What is missing from this otherwise excellent work is a sense of context, whether in terms of Ottoman Egypt or on a broader scale.

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AFRICA

CHARLES-ANDRÉ JULIEN. *History of North Africa—Tunisia, Algeria, Morocco: From the Arab Conquest to 1830*. Edited and revised by R. LE TOURNEAU. Translated by JOHN PETRIE. Edited by C. C. STEWART. New York: Praeger Publishers. 1970. Pp. xvi, 446. \$13.50.

Responding to widespread interest in the history of Africa, Praeger has released an English translation of Charles-André Julien's classic study of North African history from the Arab conquests to 1830. Embellished with excellent maps, reference notes, and a detailed bibliography, this reasonably priced volume is both a corrective to the view that Africa had no meaningful history before the arrival of Europeans and an invaluable guide to the study of precolonial North Africa.

Influenced by the geographical uniqueness of North Africa, the author organizes his history of the Maghrib around the stand taken by this region against the outside. Julien adopts the viewpoint of his Islamic sources and describes how North Africans resisted Islam from the time of the Arab conquests through the fifteenth century. Heterodox Islamic movements, therefore, appear as opposition to the Eastern invaders and not as frontier manifestations of a spreading Islamic culture. For Moroccan history between the eleventh and thirteenth centuries, this same perspective leads the writer to stress the Berber influences in Almoravid and Almohad expansions, although the animating force for both of these imperial movements was a reinactment of the Islamic drama. Even the final conversion of North Africa to Islam came not from the power of Islam's appeal but from the North African reaction to the fifteenth-century Iberian invasions.

Additional evidence of the Moroccan and Berber character of Julien's work is the importance assigned to the history of Moroccan dynasties as contrasted with the developments in the eastern Maghrib. Well over half the book deals with the western portion of North Africa, where the influence of the East shaded off and the society remained predominantly Berber rather than Arab. Tilting the history of the Maghrib in favor of Morocco results, then, in an extensive treatment of the Almohad dynasty, whereas only a few pages are devoted to the important tenth-century Fatimid Empire. Similarly, the Saadi dynasty of Morocco carries on the uniqueness of Maghribian history in the modern period, while the expansion of the far more powerful Ottoman Empire into North Africa is dismissed as having added nothing new to the area.

Although Julien underlines the autonomy of North Africa more than the Islamic history of this region justifies, his work does not support efforts to deny the Maghribians their past. Writing in the heyday of imperial enthusiasm over the conquest of North Africa, the author drew attention to the rich cultural heritage of precolonial North Africa and not to the construction of the French colonial empire. The subsequent rise of modern North African political movements underscored how much closer Julien was to the wellsprings of Maghribian history than were those who celebrated the colonial adventure. In the postcolonial age his book remains without comparison as an introduction to the Islamic history of North Africa.

ANDREW C. HESS
Temple University

PIERRE GRILLON. *Un chargé d'affaires au Maroc: La correspondance du consul Louis Chénier, 1767-1782*. In two volumes. (Bibliothèque générale de l'École Pratique des Hautes Études, VI^e Section.) Paris: S.E.V.P.E.N. 1970. Pp. 603; 606-1072. 145 fr. the set.

These volumes contain over five hundred documents reflecting details of Chénier's activities and observations. An introduction describes the consul's background and gives some understanding of the important personalities and conditions in Morocco. The diplomatic notes are a source of information for particular

transactions of mercantile houses and individual shipmasters, but fall primarily within a European frame of reference.

First, the documents reflect that the French-Moroccan treaty of 1767 was motivated by the Marseilles chamber of commerce, which sought to maintain the sultan's political and socioeconomic feudal rule while exploiting the markets in his realm. The chamber's concerns included the provision of adequate gifts for the indigenous rulers and the constant evaluation of the sultan's government's views on commerce. It also concerned itself with the sultan's military strength for maintaining law and order by limiting the troublesome Salé corsairs, checking the revolts of various princes and local governors, and suppressing the uprisings of black African subjects and military units because of discriminatory practices. Other concerns included the gathering of information on the activities of the diplomatic corps and merchants of those European states, particularly Great Britain and the Netherlands, that competed against French commerce. There was constant pressure on the sultan to open his ports, limit his trade monopolies, restore merchandise confiscated from French entrepreneurs, and permit the immigration of French workers. There were occasional references to the consul's intervention to assist persecuted Jews as well as to a sense of Muslim interstate cooperation as reflected in the sultan's protests over the bombing of Tunisian ports. The volumes reflect the classic interrelationships between European commercial imperialism and a traditional feudal state.

Unfortunately for the historian of indigenous Moroccan history, there are only a few descriptions of the conditions of various levels of the local communities. Chénier was primarily interested in the general character of the elites with whom he had to deal and not with the mass of peasant and urban laborers. Within these restricted references he criticized the lethargy and self-content of the establishment princes, not because they did not advance their subjects, but because of their exclusiveness and isolation to European penetration. The occasional energy exerted in military encounters and expeditions is decried as wasteful. Yet this collection of diplomatic notes is valuable, for new

facts are available that reflect the methods whereby indigenous elites cooperated with European economic and political interests.

GEORGE MOUTAFAKIS

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BENOÎT VERHAEGEN. *Rébellions au Congo*. Volume 2. (Les études du C.R.I.S.P.) Brussels: Centre de Recherche et d'Information Socio-Politiques. [1969.] Pp. xxvi, 830. 600 fr. B.

This book is part of a tradition of Belgian scholarship and documentation to which Benoît Verhaegen has made considerable contributions in recent years. He has been associated with Le Centre de Recherche et d'Information Socio-Politiques (CRISP) in Brussels, which published this book and which has brought together a group of Belgian scholars holding the views of the European Catholic Left. Between 1960 and 1963 Verhaegen was editor or co-editor of the massively documented and invaluable annual summaries of Congo politics that were initiated and have been continued by J. Gérard-Libois—books that indicate the concern of CRISP to understand not only the position of the Congolese government but also to deal extensively with that of dissident groups.

Verhaegen has now produced the second volume of a history of the rebellions of post-independence Congo. This particular volume deals exclusively with the region of Maniema. The author collected his material in the Congo between 1965 and 1968, in particular in Maniema, Kivu, and Kinshasa. He has had access to a remarkable amount of archival and other written sources. Nevertheless, he considers that the use of oral sources has become more and more important in such research. He, therefore, begins with a useful introduction in which he discusses his research techniques. This section is a continuation and development of a chapter in his first volume in which he dealt with the methodological problems involved in such a project. There then follows an analysis of the sociology and geopolitics of the region. But the essence of the book is to be found in the detailed discussion of politics in Maniema from about 1959 and after. Verhaegen discusses the introduction of nationalist political parties into the area and the gradual breakdown of

constitutional politics between 1960 and 1964. The latter chapters deal with the rebellions in Kasongo and Kindu. The author analyzes the structure of government established by the *simba* leaders, notably the difficult relations between the civilians and the military. The one recognized party quickly went into eclipse and was reduced to the status of supplying the wants of the army and of incessant internal bickering. He also discusses the ideology and rhetoric of the new leadership, in particular the significance of the doctrine of the second revolution evoking the name of Lumumba and promising the realization of the benefits that the Congolese politicians in Kinshasa promised before independence but had failed to produce. Equally significant for the *Armée populaire* was the use of ritual and magic. This included initiation rites along the lines of baptism and the promise of immunity from bullets, a characteristic of a number of African rebellions and resistance movements. This ritual, through daily participation in the rites, also provided a degree of cohesion for the army. The revolt was, therefore, according to the author, indigenous; it was not a long-standing conspiracy organized from abroad, although the rhetoric of the leaders was influenced by their hope for support from Russia and China.

In the early stages of the rebellion there was almost no resistance, largely because, according to Verhaegen, the majority of the people, especially among the young, supported the *simba* and because the local nationalist parties provided a framework for the organization of the revolt. The majority of the people in the region only turned against the army as a consequence of brutality and the decline of the economic situation. The first military defeats provided the opportunity for such discontent to become manifest and for the ultimate restoration of the rule of the central government. All in all this is an invaluable book both for its analysis and for its documentation. We can only await the third volume on the revolt in Stanleyville and Orientale Province with special interest.

DONALD C. SAVAGE,
Canadian Association
of University Teachers

ANN BECK. *A History of the British Medical Administration of East Africa, 1900-1950*. (Commonwealth Fund Book.) Cambridge, Mass.: Harvard University Press. 1970. Pp. xiv, 271. \$8.00.

In the continuing debate concerning the impact of European colonialism upon Africa's development, one of the strongest arguments advanced by those defending, or at least not totally damning, the European presence is the introduction and succeeding benefits upon Africans of European medical science. Thus Ann Beck's volume is welcome, particularly since she is generally correct in affirming that most scholars have paid little attention to the impact of European medical services on East African subject peoples.

The author's announced intention is to treat her topic as part of the overall history of East Africa during the years between 1900 and 1950. In general Beck has succeeded. There are few surprises in her conclusions that medical policies "were affected by the prevailing trends in the political interpretation of the duties of the colonial power"; that the medical services were influenced by "changing concepts of welfare in modern society and new ideas on education in tropical countries" (p. 198). It is also good to be reminded that private medical missionaries long played a vital role while governments neglected the task, and that the care of Europeans in East Africa was the foremost task of the early medical services. The latter concern changed after World War I, at least in administrative rhetoric, but the services given to Africans were often curtailed by the necessity for colonial governments to remain within their budgets. There have been serious criticisms of the slowness of developments designed to aid Africans in the years prior to 1960, but the author openly decides to let this point pass in silence: "This study does not attempt to evaluate what ought to have been done. It limits itself to the reconstruction of the history of British medical administration in East Africa" (p. 208).

Within these limits Beck has given us a straightforward account that fills an important gap in the modern history of East Africa, especially for Kenya, the main focus of the volume. There are some minor errors of detail in the general account of events, but they do

not greatly detract from the narrative. The greatest fault, as I see it, is the author's dry-as-dust recounting of what could have been a gripping account of the battle against the many diseases endemic to East Africa, a battle often carried on in bush stations with inadequate medical facilities. The author did make the effort to visit modern bush facilities to acquire a feeling for this aspect of the work; however, the effects of the visits do not penetrate into the style of the volume.

Nevertheless, Mrs. Beck's effort will stand as a useful introduction for further work on a neglected theme of East African history.

NORMAN ROBERT BENNETT
Boston University

ROBERT L. HESS. *Ethiopia: The Modernization of Autocracy*. (Africa in the Modern World.) Ithaca: Cornell University Press. 1970. Pp. xx, 272. \$8.95.

Dr. Hess's recent book on Ethiopia, an expanded version of a previously published essay, belongs to a popular yet scholarly vein of Ethiopian historiography: his book is directed to a general public and is not intended to present a definitive contribution to the field; it attempts rather to fill a general lack of information about important aspects of modern Ethiopia by grouping together a considerable amount of little known facts and figures.

The book could be divided in two parts: the first introduces the reader to the general setting of the country and its peoples and offers a brief survey of "more than two thousand years" of history, while the second part, the core of the book, discusses at some length such topics as political stability, national integration, the unsuccessful coup in 1960, the role of the students, and the end of Ethiopian isolation through a progressive identification with the rest of the African continent. A brief final chapter concludes the book by speculating about the problems of dynastic succession and the more general future of the country.

A certain number of generalizations, perhaps unavoidable in a book of this nature, are nonetheless questionable. Such, for instance, is the suggestion running throughout the historical section that Ethiopian history "since

the late thirteenth century" is the history of the Shoon dynasty. In this light Ethiopia is seen as a monolithic country, centered around one dynasty, progressively growing from the time of the "Solomonic restoration" onward. Thus Ethiopia "ceased to exist" during that period known as the Age of the Princes (1769-1885), when central power "disintegrated," only to find its moment of glory at the turn of the century when the empire "reconstituted itself under the strong leadership of Menelik II.

This theory, though traditional, is not totally convincing in that it fails to explain the composite nature of the Ethiopian polity throughout the centuries and the country's fundamental change from a tributary to a unitary state that took place only at the end of the nineteenth century. The identification of Ethiopia as the Christian "land of the Queen of Sheba," a necessary corollary of this theory, is thus unfortunately maintained for a country of which about half of the population is either Muslim or pagan.

Similarly, to group together rulers such as Theodore, Yohannes, and Menelik as strong innovating and modernizing emperors is to confuse, in my judgment, modernization with centralization and to identify innovation with that measure of social change that allows the basic structure of the state to reinforce itself. This is also true of the present emperor, whose undoubted leadership has produced at the same time the "major impetus" for change but also its major source of control. The character of Ethiopian leadership is thus exemplified by the book's subtitle, "The Modernization of Autocracy," and its inherent limits.

Hess's book is a commendable effort to make Ethiopia a little more familiar to the American public. An annotated bibliography and a good index make this book a useful work of reference that, it is hoped, may be further improved by future revisions.

ALESSANDRO TRIULZI
Northwestern University

D. J. MURRAY. *The Governmental System in Southern Rhodesia*. New York: Oxford University Press. 1970. Pp. xxi, 393. \$13.00.

This book studies Rhodesian politics between 1923 and 1953, a limitation not suggested by

its title. It was a period of marked change in Southern Rhodesian politics. Flushed with their new autonomy, settler administrations after 1923 developed a formally racially segregated society. Legally and administratively, they segregated African land and education. They instituted African councils and elevated chiefs to deflect growing African aspirations. They crushed African threats to their urban and rural economies. If the Central African Federation (1953-63) marked a partial lull, the advent of the Rhodesian Front party in 1962 heralded the resumption of the earlier program.

This study analyzes the interaction of both white occupational organizations and African associations with the government. The organizations sought power for their political ends, while the governing party sought to implement policy through the organizations. The central thesis is that the cohesion of white politics, and in particular the long tenure of Premier Godfrey Huggins (1933-53) cannot be explained by the whites' fear of Africans. Huggins adroitly traded favors with pressure groups, retaining political power at the expense of consistency. But by the 1950s the occupational organizations were unrepresentative. A differentiated political party emerged, appealing to whites alarmed by the rise of organized militant African political consciousness, and the "Huggins system" collapsed.

These conclusions rest on an intensive study of published sources, including the less accessible publications of the predominantly white pressure groups. The analysis is valuable, but limited. The central thesis, persuasively if somewhat turgidly argued, provides an often novel interpretation of the development of Rhodesian politics. But the author should have used two additional categories of sources. The accessible archival sources, hardly consulted, conclusively demonstrate the extent to which white politics were dominated by fears of African advancement, primarily in land, agriculture, and politics. These fears were less readily expressed in print. Use of these sources would have provided material to analyze the actual processes of reaching key policy decisions and of deliberately excluding Africans from governmental power. The other untapped source—crucial in

such a tiny political community—includes the reminiscences and papers of the participants, who here are usually shadowy figures obscured by monolithic organizations. Such sources also undermine the myth—scarcely questioned here—that during most of this period Africans were generally acquiescent under colonial rule. Thus the book is a substantial, but not nearly definitive, contribution to our knowledge of Rhodesian white and African politics.

J. KEITH RENNIE
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ASIA AND THE EAST

RENÉ GROUSSET. *The Empire of the Steppes: A History of Central Asia*. Translated from the French by NAOMI WALFORD. New Brunswick: Rutgers University Press. 1970. Pp. xxx, 687. \$17.50.

The Empire of the Steppes chronicles the history of inner Asia from antiquity until 1757, the last year of the Jungar Empire. The first third of Grousset's epic treats pre-Genghisid leaders, battles, and territorial changes, with forays into the world of philological problems and oriental art. The author describes chronologically the Scythians, Huns, Hsiung-nu, T'u-chüeh, Uighur, and Seljuks, as well as the Kara-khitai and Kwarizmian empires.

This first section builds toward the Elizabethan period, so to speak, of nomadic history—the times of Genghis Khan and Tamerlain. For these two hundred years (1200-1405) the political story is full and accurate, though René Grousset acts as *defensor fidei* (Christianity in various forms), portraying Moslems as fanatics and overemphasizing the role of Nestorian Christians. Institutional history, cultural adaptations, and the theory of sedentary-steppe relations receive little attention, but the author leads us carefully through the almost indigestibly prolix political details that characterized the declining Genghisid empires and the rise of Tamerlain. René Grousset regards Tamerlain as an educated villain who, unlike the "noble savage" Genghis, should have known enough to act more decorously. Tamerlain's military campaigns are carefully and concisely described, but the conqueror's

more sophisticated and constructive side is neglected. *The Empire of the Steppes* does not mention Tamerlain's policy of keeping his nomadic troops busy and happy by promoting constant warfare outside of the home area, Transoxania, thereby enabling this area to flourish. Unlike Genghis Khan, who drove sedentary wealth out of his empire, Tamerlain in his time achieved peace and enrichment at home. To balance Grousset's portrait of Tamerlain, one must also say that Tamerlain was intelligent enough not to give his nomads arable land and that he knew how to promote trade. The description of the fifteenth-century Timurids underplays the dervish activities, omitting entirely Khoja Ahrar, the virtual ruler of late fifteenth-century Transoxania, and does not note the significance of the dervish anti-intellectualism that was to stifle Central Asia.

The final section covers the decline of the Golden Horde, the rise and fall of the Jungar Empire, and the chaos in Transoxania. Though current research has added to what René Grousset knew in his lifetime, his account is basically accurate. The description of Jungar-Tibetan relations of 1700-20 is somewhat unbalanced, and the Mongol rebellion of 1756-57 is not mentioned. More important, however, is the lack of a comprehensive explanation of the decline of nomadism and of inner Asia. *The Empire of the Steppes* ends without a recapitulation and coda.

Though some of the material needs updating and the pace of the book straddles uncomfortably the gap between the layman's and the expert's interests (for example there are copious footnotes and philological details, but only sources available in Western European languages are cited), it is to be recommended as a complete and accurate general history of the steppe empires.

FRANK HUDDLE, JR.
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EDWARD H. SCHAFER. *Shore of Pearls*. Berkeley and Los Angeles: University of California Press. 1970. Pp. ix, 173. \$7.00.

Hainan Island, the subject of this fascinating study, is located between the China Sea and the Gulf of Tonkin and separated from the

southern coast of mainland China by the Hainan Strait. In six informative chapters Professor Schafer discusses the history, natural aspects, aboriginal tribes, commerce, Chinese exiles, and varying foreign opinions of Hainan. Drawing on an impressive array of Chinese and Western sources, he reconstructs the Chinese image of the island and its peoples from earliest antiquity to the end of the Northern Sung dynasty. Even a charitable interpretation of these sources reveals that the Chinese held the native aborigines, the Li people, in low esteem and exploited them ruthlessly. Apparently the lush vegetation and exotic fauna of Hainan had little appeal for the Chinese; it was the riches to be gained from such goods as aromatic aloeswood and gold that attracted merchants, government agents, and adventurers to the island.

Throughout the period covered by the book Hainan was a dreaded place of exile for Chinese officials in disfavor. The most famous of these hapless exiles was Su Shih, the Sung dynasty official, poet, and calligrapher. While Su Shih praised the tea made from the water of Hainan's fresh springs and found some intellectual companionship among the natives, less resourceful officials were driven to suicide or mental decay on the Shore of Pearls.

In his introduction Professor Schafer modestly describes this book as an "inflated footnote" of his earlier study, *The Vermilion Bird* (1967), which itself narrowed the focus of *The Golden Peaches of Samarkand* (1963). Let us hope he will continue to delight and inform readers with still further studies of contacts between the Chinese and the world beyond the central kingdom.

THOMAS LAWTON
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WM. THEODORE DE BARY and THE CONFERENCE ON MING THOUGHT. *Self and Society in Ming Thought*. (Studies in Oriental Culture, Number 4.) New York: Columbia University Press. 1970. Pp. xii, 550. \$17.50.

Self and Society is a fully packed Chinese chest. First we find five papers on Confucian thought of the Ming dynasty (1368-1644), ranging from a pioneering, highly informative piece by Wing-tsit Chan (to whom the volume

is dedicated) on neglected but influential early Ming thinkers, to a rich and perceptive study by the editor of late Ming philosophy after Wang Yang-ming. Professor De Bary's portrayals here of the brash and impulsive Wang Ken and the individualist Li Chih, who died a martyr for his deviations, are unsurpassed. The other papers in this group are by contributors from the Far East, including the well-known scholar Tang Chun-i. Jen Yüwen's is new information, in quantity and with much close analysis, on the fifteenth-century southern meditative moralist Ch'en Hsien-chang. Professor T'ang and Professor Takehiko Okada of Kyushu University offer essays (to which I will return) on conceptual problems in the Wang school of "mind."

If this first group of papers shows us the underlying unity of Ming philosophy, the marked turning inward of Confucian psychological and moral reflection, the last seven give an impression of the great variety of Ming intellectual life at different social levels. Two deal with the Taoist's perennial preoccupation with physical immortality. Anna Seidel examines the shaping of the legend of an early Ming reputed "immortal"; Liu Ts'un-yan opens up the difficult complexities of regimens of immortality-seeking and is able to show us something easy to forget, that the most prominent and respectable of Ming Confucian moralists, even Wang Yang-ming himself, were thoroughly familiar with these mysteries. There are two careful papers (by Ray Huang and Robert Crawford) on "scholar-statesmen," which give important balance to the book. Professor Crawford's major study of the sixteenth-century Chang Chü-cheng contains unexpected rewards, for example an engrossing ten pages on Chang's philosophy of history. Especially enjoyable reading is found in Professor C. T. Hsia's sensitive examination of the themes of time, change, and mortality in the romantic plots of the late Ming dramatist T'ang Hsien-tsu. Professor Tadao Sakai's article "Confucianism and Popular Educational Works" not only gives us important material on the intersection of philosophy and religion at a different level of society but serves also to introduce the research of a Japanese scholar whose work deserves to be better known.

Leon Hurvitz' "Chu Hung's One Mind"

is a valuable ground-breaking study of one of the last examples of a living tradition of Buddhist philosophy in China and is presented with commendable clarity. Confucian preoccupation with Buddhist texts and themes—for example, in Li Chih, Chang Chü-cheng, and Wang Chi—is briefly treated in other papers, but the relationships between Buddhist and late Confucian mental self-cultivation are in need of a closer and fuller analysis than they receive, in this book or in any book. Research in this difficult area continues: two papers on late Ming Buddhists were presented in a conference recently organized by Professor De Bary, which may later yield a sequel to the present volume.

Two of the articles that interested me most in the opening group of philosophical papers are the re-examinations of Confucian philosophy of mind by Professors T'ang and Okada. They are not easy reading. To me, T'ang's is sometimes unclear, and Okada's often more so; indeed one must be quite familiar with the Chinese material he is dealing with to find him anything but opaque. But Professor T'ang's "Concept of Moral Mind" is rewarding. He offers, convincingly, a fundamental reinterpretation of Wang Yang-ming as synthesizing the Sung schools of Chu and Lu (rather than merely developing Lu's thought), and in doing so he has shown me a world of insight in the Sung and Ming Confucians that I had not realized was theirs.

I have not gained as much, yet, from Professor Okada's "Wang Chi and the Rise of Existentialism." I do not look forward to the time when I must choose whether to dismiss his article as unprofitable or try to explain it to my students. I think he means by "existentialism" the thesis that if we wish to grasp what our mind is we have to observe what it is doing; that is, that the mind's "substance," typically in late Confucianism identified with a world-ordering "principle," is not something lying behind a person's play of mental act, emotion, and moral effort, but that play itself. But I am not sure of this. I do not think this is quite what "existentialism" has meant to other people, but perhaps it implies at least some genuinely existentialist themes. The problem is worth much more study.

It is regrettable that a book as important

and as valuable as *Self and Society in Ming Thought* has been priced beyond the reach of many students who will want it. Investigation has persuaded me that the publishers have charged what they had to; and, indeed, if it had been published this year the price would have had to be higher. But we must all ask what this continuing pressure of cost may do to scholarship.

DAVID S. NIVISON
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MARY E. FERGUSON. *China Medical Board and Peking Union Medical College: A Chronicle of Fruitful Collaboration, 1914-1951*. New York: China Medical Board of New York. 1970. Pp. 263. \$7.00.

The Peking Union Medical College was the foremost Western established medical institution in China and probably the most enduring monument to the century of foreign missionary and philanthropic enterprise that ended in 1949. This book is an administrative history of the college (generally known as the PUMC) in its relations with the Rockefeller Foundation and the New York-based China Medical Board, set up to administer the foundation's medical programs in China.

There are severe limitations to this type of history, and it cannot hope to bring out the full significance of the remarkable institution it discusses. For instance, the important scientific and medical work at PUMC, including such basic discoveries as synthesis of the drug ephedrine and Davidson Black's paleontological work on the Peking Man, is only touched upon. The impact of PUMC and its graduates on the modern Chinese medical profession can only be inferred from occasional references to activities of the faculty and graduates. The Chinese response to this foreign institution in a period of increasing nationalism—the tension between appreciation for its scientific contribution and suspicion of its foreign control—is treated only incidentally.

Yet the book is livelier and more candid than many official histories of this type. In treating the frequent tensions between the board in New York and the college's administrators and trustees in China the author, who was a long-time executive secretary of PUMC, reveals some interesting aspects of the financing and

the direction of American philanthropic enterprise abroad. Most interesting is the "irregular" personal intervention of John D. Rockefeller, Jr. to effect the removal of the acting director of PUMC, Roger S. Greene, partly because of differing opinions on the role of Christian evangelism in the college. This incident provoked what was probably the sharpest disagreement between the college's predominantly Chinese board of trustees and its American financial backers.

On the whole, however, the history of the college was marked by a high degree of American-Chinese cooperation despite the financial problems of the Great Depression and the vicissitudes of twentieth-century Chinese domestic and international politics. Only after the outbreak of the Korean War and the State Department's freezing of all financial transactions with China was the PUMC nationalized in January 1951.

There runs throughout the book a quiet note of pride in fulfilling John D. Rockefeller, Jr.'s original promise to "make permanent the establishment on Chinese soil of the best in scientific medicine that the world can offer." The PUMC did this, as proven by the college's continued existence as the leading medical center in the Peoples Republic.

Many of the hopes of its founders—such as the wish of former president of the Rockefeller Foundation, Raymond Fosdick, that medicine will be one of the ties "that bind the human race together regardless of ideologies and boundary lines"—may now seem rather dated and perhaps naive. But of all the blasted dreams and withered hopes from the era "when China faced West" the efforts of those who built the PUMC seem to have come closest to bearing fruit.

RALPH C. CROIZIER
University of Rochester

JEROME B. GRIEDER. *Hu Shih and the Chinese Renaissance: Liberalism in the Chinese Revolution, 1917-1937*. (Harvard East Asian Series, Number 46.) Cambridge, Mass.: Harvard University Press. 1970. Pp. xiii, 420. \$12.50.

This is a distinguished book from a distinguished series. Drawing upon Hu Shih's published and unpublished writings as well as personal interviews with him before his death in 1962, Pro-

fessor Grieder of Brown University has written an illuminating and sensitive biography of one of modern China's leading scholars and its most prominent exponent of liberalism. He deals primarily with Hu's social and political views (rather than his works of scholarship) and rightly focuses upon the two decades 1917-37 when those views had the most influence. Mr. Grieder treats his subject sympathetically though not uncritically. He analyzes Hu's ideas with care, confidence, and subtlety, showing the remarkable continuity of his thought and comparing his ideas with those of his mentor John Dewey, his contemporaries in China, and also the Confucian tradition (which at a subconscious level was more influential than one would have expected in a man who made his reputation attacking that tradition). It is a beautifully written book about an important personage.

Mr. Grieder suggests that liberalism in modern China was doomed to failure because the environment was so inhospitable, that Hu's dilemma as a liberal living in a revolutionary society was basically unresolvable. Yet it seems to me that, in a small way at least, Hu Shih, as a leader of the liberal bloc, contributed as well to its failure because of his own personal limitations. Hu's flaw was not that he was elitist, for many of his contemporaries, not excluding the communists, were also elitist, but that he was too cerebral in his approach to social and political questions. He repeatedly urged his students and readers to stop ranting about "fundamental solutions" like socialism and instead to study "specific problems" such as the standard of living of the ricksha coolie. But how much did Hu himself study these problems? More important, how much did he do to help alleviate the coolie's plight? Precious little, one gathers. Hu was a strikingly unemotional man, characterized by what Mr. Grieder describes as "moral blindness to the dimensions of the suffering that afflicted his countrymen." Thus, in his hundreds of magazine articles he referred to the problems of the peasantry hardly more than once or twice! As someone in the early 1930s commented bitterly, Hu Shih "never scratched where it itched." Nevertheless, in his chosen stance as a detached and dispassionate critic, he stuck steadfastly, even heroically, to his liberal values

in the face of opposition from all sides. He was, to the end, a good man. But as he himself had exclaimed in a brief and uncharacteristic moment in 1920, "It is not enough to be a good man—it is necessary to be a good man who can fight." We have much to learn from this study of Hu Shih's life.

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A. B. BLAGODATOV. *Zapiski o kitaiskoi revolyutsii 1925-1927 gg.* [Notes on the Chinese Revolution of 1925-1927]. Moscow: Izdatel'stvo "Nauka." 1970. Pp. 250.

In the wake of deteriorating Sino-Soviet relations, the USSR in the late 1950s began publishing materials on the Russian revolutionary experience in China during the 1920s. Lieutenant-General A. B. Blagodatov's book is one of the latest examples of such efforts.

Blagodatov (known as Rollan in China), who ended up as Blyukher's chief of staff and deputy, was in China for two and a half years before departing for home in July 1927 following the collapse of the Wuhan regime. Some of what Blagodatov writes has been covered by other memorialists, particularly by M. I. Kazanin, who was Blagodatov's translator in 1926-27 and who detailed his experiences in *V shtabe Blyukhera. Vospominaniya o kitaiskoi revolyutsii 1925-1927 godov* (1966). But Blagodatov specifically states that he does not want to repeat the observations of others and generally avoids giving his version of situations covered elsewhere in recent Soviet literature. His eyewitness reports of the collapse of the second national army in early 1926, of the journey of the Kuomintang government from Canton to Wuhan later that year, and of affairs at Chiang Kai-shek's headquarters in Nanchang and Nanking in 1927 fit his criteria of nonrepetitiveness and are important for Western scholars.

In addition to his own diaries and recollections, Blagodatov uses undesignated archives and the journals of other Russian advisers in China. Among the new materials he presents from these latter sources, the account drawn from a stenographic report of Feng Yu-hsiang's meeting with Russian government officials in Moscow on March 21, 1926, is the most significant.

Blagodotov does best when he draws upon his own resources or directly from materials such as those cited above. When he attempts to outline the broad picture, his grasp seems rather limited. Nevertheless, his recital of the steps in the collapse of the "Left" Kuomintang, though scarcely new or penetrating for non-Soviet observers, will fill in many previously existing gaps for the Soviet reader.

Zapiski o kitaiskoi revolyutsii 1925-1927 gg. is not so important as the previously published, more comprehensive two-volume memoir of Lieutenant-General A. I. Cherepanov (*Zapiski voennogo sovetnika v Kitae* [1964]; *Severnyi pokhod natsional'no-revolutsionnoi armii Kitaya* [1968]), but it has its own attractions and makes its own not insignificant contribution.

DAN N. JACOBS
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CHIN-TUNG LIANG. *The Sinister Face of the Mukden Incident*. (Asia in the Modern World, Number 6.) New York: St. John's University Press. 1969. Pp. xi, 188. \$3.75.

This monograph adds documentation in support of the thesis that scheming Japanese officers, particularly those of the Kwantung Army, brought about the Mukden incident as a major step in a grand conspiracy to impose on Japan a military-dominated, fascist regime. According to Liang the 1931 Mukden spark eventually kindled World War II.

Taking on the mien of the judge in Marc Bloch's discussion of historical analysis, Liang underscores his thesis of Japanese culpability by drawing on heretofore unavailable Chinese and Japanese sources, principally secondary works and the archives of the Japanese foreign ministry, as well as the proceedings of the International Military Tribunal for the Far East. Liang's legal training is manifested as he ferrets out of the maelstrom of army politics those "sinister figures" responsible for the incident and subsequent depredations against China.

In six uneven chapters the author provides a background to the incident, a glance at the incident, details of the Mukden conspiracy, discussions of the reactions of Tokyo and Nan-king, and his ideas on the causes of the incident. Too little space is given to his hypothesis

that the "real" causes of the Mukden incident may have been first, a subtle shift of naval strength in the Pacific; second, fluctuations in Sino-Soviet relations; and third, the hopelessness of the Manchurian-Mongolian separatist movement.

Although Liang has brought to English-language print a wealth of heretofore unavailable documents, the overall quality of this work is materially affected by the numerous errors in the narrative, appendix, and bibliography, especially in the transliteration of Japanese names into English.

G. RALPH FALCONERI
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MARTIN EBON. *Lin Piao: The Life and Writings of China's New Ruler*. New York: Stein and Day. 1970. Pp. 378. \$10.00.

This journalistic account of Mao Tse-tung's designated successor is written for the general reader. The book has two parts, a biographical essay and a selection of Lin's writings. Information on Lin himself comprises only a minor portion of the first part, the balance being filled in with sketches of other members of the Chinese Communist ruling elite. Large blanks occur in the author's rendition of Lin's life, despite the existence of numerous sources that adequately fill the gaps and explicate many of Lin's actions. The author, who does not read Chinese and who is relatively unfamiliar with Chinese Communist history, thus relies on his own speculation (treated, unfortunately, as fact) or on extraneous detail to smooth over the bumps.

The second section comprises some of Lin's writings from 1940 to 1969. While some important selections are included (the 1965 people's war speech and Lin's Politburo speech on politics of May 1966), the impression is created that the compendium is complete. Most of Lin's significant writings, however, appeared before 1940 and are available but not included in the book. The 1940 article that begins the author's compendium was probably not even written by Lin, the important declarations in the internal army publication, *Work Correspondence*, for 1961 were neglected by the author, and even the revealing speech of August 9, 1967, following the Wuhan incident was omitted. In their places the author

has put a series of purely *pro forma* greetings and messages to foreign parties issued over Lin's signature but not attributable to him personally.

Until a more definitive work on Lin appears the reader would be best advised to rely on a combination of official Chinese Communist, Nationalist, and Western writings on Lin. For the first, see *Chairman Mao's Successor—Deputy Supreme Commander Lin Piao* (1970), available from the Association of Research Libraries, Washington, D.C. A good example of the second is *The Current and Past of Lin Piao* (1968), obtainable in translation from the Rand Corporation, Santa Monica, California. An accurate and accessible example of Western writings is Loren Fessler, "The Long March of Lin Piao," *New York Times Magazine*, September 10, 1967.

THOMAS W. ROBINSON
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RICHARD T. CHANG. *From Prejudice to Tolerance: A Study of the Japanese Image of the West, 1826-1864*. (Monumenta Nipponica Monograph.) Tokyo: Sophia University. 1970. Pp. xiv, 237. \$6.50.

This book is basically an analysis of the response of two important warrior-scholars, Fujita Tōko (1806-54) and Sakuma Shōzan (1811-64), to the nineteenth-century pressures from the Western powers. In his analysis the author makes a special contribution to intellectual history and presents new and interesting glimpses into the lives and thought of these men and of their times.

They are shown to have had much in common. Their writings reflected the Confucian tradition, especially the principles of Sung Tzu as set forth in his treatise on war, *Ping Fa*. They were influenced by their knowledge of China's defeat by England in the Opium War. Through their study of Dutch writings they came to recognize the superiority of Western technology and military power. They were both imprisoned by the Tokugawa dictatorship for their subversive activities. On the other hand, their writings clearly show that they developed different views of the West.

Fujita, who served as a close adviser to his feudal baron, Tokugawa Nariaki of Mito, a

leading advocate of exclusion, was violently opposed to foreign trade. He also feared that Christianity would be used as a ruse by the foreign powers to lure Japan into submission. He saw little differences among the Western powers. They had superior military strength, were unreliable, acquisitive, and hence Japan's enemies. In the two years prior to his death he organized the construction of large Western-style ships and reverberatory furnaces. Sakuma, who was an official of a fief in central Japan and had spent several years in Edo (old Tokyo), was more sophisticated about the West. He recognized that the European countries varied considerably. While he was fearful of England's designs, he held Holland in high esteem and recommended that it be asked to help Japan to improve its defenses. He urged avoidance of war until Japan's defenses were improved, yet advocated foreign trade as a means of obtaining intelligence about potential enemies. He often referred to the superiority of the technology of the West and of the ethics of the East. He became an authority on Western-style gunnery and opened a school in Edo where he taught several of the most brilliant young nationalists. An adviser on foreign affairs for the Tokugawa government, he was assassinated while trying to persuade the imperial partisans to form a united front against the foreigners.

The shifts of these two protagonists in their images of the West appear, however, to have been from ignorance to awe or admiration rather than from "prejudice to tolerance." The book would be far more useful as a ready reference were it not for its confusing index.

HUGH BORTON
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GEORGE ALEXANDER LENSEN. *Japanese Recognition of the U.S.S.R.: Soviet-Japanese Relations, 1921-1930*. Tokyo: Sophia University, in cooperation with Diplomatic Press, Tallahassee, Fla. 1970. Pp. 419. \$15.00.

The course of Professor Lensen's published peregrinations through Russian relations with Asia has now "progressed" to the immediate postrevolutionary period and focuses here on the process of accommodation between the Soviet Union and Japan. The story is told chronologically, seemingly without overlooking

the least detail, in fact so exactly that most of the numerous relevant documents and even variants of these are reproduced in full in the text itself. Lensen suggests in the preface that he reprints the documents in order to stress his point that the difficulties of the 1920s in Japan's recognition of the USSR are of "more than academic interest today," for in this case study he sees possible parallels not only in contemporary Soviet-Japanese relations but in Sino-Japanese and even in Sino-American relations.

One assumes that the significance of such parallels relates to broader questions of national interests, economic systems, styles of diplomacy, and the like rather than to the minutiae of oil, coal, or fishery negotiations between the parties. If this assumption is correct then the first five chapters of Professor Lensen's book, which detail all the nuances of the five Soviet-Japanese conferences (1921-25) that ultimately resulted in the Basic Convention of 1925 and Japan's recognition of the USSR, are much more successful than chapters six through eleven, which are obviously intended to round out the record. In fact pages 177-95 of chapter 6 simply reproduce the Basic Convention verbatim, and pages 271-316, all of chapter 10, provide a similar verbatim text of the Fishery Convention between Japan and the USSR.

Once more, in chapter 12, entitled "Lingering Mistrust," and in a subsequent "Epilogue" Professor Lensen returns to some of the intriguing questions of interpretation that his research raises and, in what may be his most useful analysis, he contends that "the Japanese trusted the Soviets as little as the Soviets trusted them" (p. 344). Nevertheless he demonstrates convincingly that such mutual distrust did not override mutual opportunism, which each country in its own way saw as improving its international position vis-à-vis the United States. What seems distressing is that in an excess of attention to very specific data some of the provocative insights that Professor Lensen has are too often given short shrift in favor of details that might well have been relegated to footnotes or appendixes.

A further problem for Professor Lensen is the kinds of sources on which he has relied. In his footnotes there seems to be a certain

undifferentiated use of diverse material with particular reliance on the Japanese foreign office archives, Tanaka Bunichirō's *Nisso Kōshōshi* (History of the Negotiations between Japan and the Soviet Union) (1942), and *Dokumenty vneshnei politiki SSSR* (Documents of the Foreign Policy of the USSR), published in 1958 by the ministry of foreign affairs of the USSR. It would have been extremely useful for the reader if at the outset Professor Lensen had provided his own evaluation of the reliability of at least these three principal sources as well as an explanation of his method of utilizing them. For example, the Japanese archives he uses were seized by the American-led occupation and microfilmed for the Library of Congress; the Tanaka volume was a secret report prepared for "in-house" use by a former Japanese ambassador to the Soviet Union; the *Dokumenty* is an official publication of the Soviet government. Surely each of these has a particular value to a historian of Professor Lensen's sophistication, and his readers are entitled to be informed accordingly.

GRANT K. GOODMAN
University of Kansas

AMERICAS

DANIEL M. MENDELOWITZ. *A History of American Art*. 2d ed.; New York: Holt, Rinehart and Winston. 1970. Pp. x, 522. \$17.95.

This attractive work offers an overview of the arts in America from precolonial times to the present. Discussions of painting, sculpture, and architecture form its core, but the author also analyzes textiles, ceramics, home furnishings, and industrial design. The coverage of architecture and design, both public arts, is best; that of painting and sculpture holds no surprises. In an effort to include everything, the author sometimes slights intriguing subjects, and too much of the book consists of brief biographical summaries of artists. But the author is right to celebrate the diversity and vigor in American art.

Given that general attitude, it seems a shame the author did not adopt a wider approach. The fine arts offer rich material for intellectual and cultural historians, and the best art history uses broad themes to integrate the arts and culture, without overlooking the distinctive

qualities of individual artists and of eras. The fine arts are important and interesting for obvious esthetic reasons. But they also give clues to the needs and forces behind public tastes, to the moods of an era, and to the rate of cultural change. The canons by which critics judge works of art, for instance, can say a good deal about social aspirations and apprehensions. The education of artists and their work in professional organizations are aspects of cultural interchange among nations that need more study. The role that various groups assign the arts in a democratic society is one way of perceiving the pressures for both change and stability within that society.

Professor Mendelowitz has chosen a more strictly chronological approach, though he dots the text with useful insights. He offers more than a general textbook, but something less than a well-integrated analytical survey of the arts in American life. There is material aplenty in his pages to intrigue those interested in an art history that does not divorce esthetics from culture.

H. WAYNE MORGAN
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Austin

A. V. EFIMOV. *SShA: Puti razvitiia kapitalizma (Doimperialisticheskaia epokha)* [USA: Paths of the Development of Capitalism (Pre-imperialist era)]. (Akademiia Nauk SSSR, Otdelenie Istorii.) Moscow: Izdatel'stvo "Nauka." 1969. Pp. 694.

No matter what its overall qualities may be, a book that, among its other errors of fact, refers to Carl Schurz as a "slaveholder" (p. 160), that credits Morse with having invented the "wireless telegraph" in 1842 (pp. 245, 251), and that mistranslates the Declaration of Independence to enumerate among the "unalienable rights" that to the "control of property" (p. 368) is not likely to persuade the American reader of its right to be taken seriously. This is in some degree unfortunate, for Mr. Efimov is one of the deans of the yet rather small band of Soviet Americanists and has done much to train students who have already contributed a number of interesting works in the growing body of Soviet historical literature on America. Indeed, the present volume indicates that Efimov represents a point of view on American

history that deserves attention on our part to his efforts to apply Marxian terms of analysis to this country's past. He has chosen, however, to present his argument in the form of a practically unchanged reprinting of his *K istorii kapitalizma v SShA* (On the History of Capitalism in the U.S.) of 1934, which contains many errors of fact or of proofreading and which, as far as the footnotes bear witness, rests almost entirely on those sources then available to Efimov but that have since been extended or modified by later research.

He also includes his *Ocherki istorii SShA. Ot otkrytiia Ameriki do okonchaniia grazhdanskoi voiny* (Outline of the History of the U.S.: From the Discovery of America to the End of the Civil War), first published in 1958. This section, though somewhat freer of school-boy howlers than the first, often seems remote and schematic in its analysis of American events, to the point that one must ask if Mr. Efimov is not shaping his conclusions at a certain philosophical distance from the evidence. As but one example of this, it may be noted that the author on page 473 praises the policies of the Russian American Company toward the natives of Alaska as "at the same time to a certain degree giving them access to higher forms of production and of culture," while American policies toward the Indians are never granted even this slight relief from blanket condemnation. Other sections seem frequently to reflect the "ought to be" of Soviet Marxism without supplying all the points of fact that would seem convincing to the nonadept in that form of belief. Thus Mr. Efimov's volume, despite strivings after a breadth of approach that American scholars should find of great interest, fails both because of an oversupply of irritating factual errors and because of a marked separation between its conclusions and the strength of the facts he marshals in their support. It is unfortunate that thirty-five years of work in the field of American studies could not have shown to better advantage than it does in this book and that Mr. Efimov's pioneering steps as a trainer of Soviet scholars must be shadowed by so faulty a presentation of his point of view.

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CHARLES A. BARKER. *American Convictions: Cycles of Public Thought, 1600-1850*. Philadelphia: J. B. Lippincott Company. 1970. Pp. xix, 632. \$15.00.

This book is a spacious and leisurely summary of what Professor Barker calls "public thought" up to 1850. His prefatory definition of public thought as the "main ideas and impulses, loyalties and traditions, which have concerned public-community life in America" is an explanation of his field of work that is obviously blurred. Much later on he approaches his field another way, implying that his work concerns the thought of people who have "been honored, from their time to ours, because they voiced and served a program which the people have cherished, which added new value to the American system," people who "became first the spokesmen, then the symbols, of lines of thought which have endured" (p. 292). This strategic conception becomes as clear as it ever does in this book in the manner typical of historians—by example. Barker's work concerns the thought of men like Adams, Jefferson, Hamilton, Marshall, Madison, Jackson, Calhoun, Emerson, and Thoreau within his time span; and Lincoln, William James, John Dewey, and Franklin Roosevelt beyond it. The book is structured around political thought, reforming religion, economic conceptions, and educational policy.

American Convictions ranges widely and frequently shows the social relationship between different categories of human effort. Barker is especially successful in at least outlining the dialectic between academic curriculums and social theory, but somewhat less successful in conceptualizing popular religion in this necessary way. A danger intrinsic to a broad historical survey, even one so leisurely as this, is occasional superficiality, as the two pages devoted to an explication of Jonathan Edwards' *Freedom of the Will* demonstrate. This weakness does not appear in the treatment of the political thinkers or popular religionists, but it seems to be inevitable in the discussion of systematic thinkers and creative writers. There are thus admirable portraits of George Whitefield and John Woolman, for example, but thinner ones of Roger Williams and James Fenimore Cooper. For all the expansiveness

of this book, there is a sense of hurry when the subject is particularly complex. But this perhaps is a consequence of stressing public thought rather than the intellectual context within which this thought about the public occurred. In the treatment of Williams, for example, unless his use of typology is at least raised, his attitude about civic and ecclesiastical pluralism will not be understood in either its fullness or its particularity. This may be a necessary price of casting so wide a net, but there are many compensations in *American Convictions*.

The "philosopher-statesmen" of the Revolution come alive because the relationships between categories are firm: politics is related to religion, and both are sometimes related to education, especially apt of course for Jefferson. The several religious awakenings are portrayed with sensitivity, but in their social rather than their theological dimensions. Yet there are important transitions that seem to explain more than they do. It is difficult to know precisely what we are being told when Barker says that "it is an open secret that the decision for American independence from England in 1776 owes much to the intellectual movement we seek" (p. 191), that is, the Enlightenment; the debate on this interesting question centers around how much is much. And what exactly is intended by what appears to be an interesting notion, that Pennsylvania "tried to bring politics into close line with doctrine and reached beyond firm grasp" (p. 272)?

Although Barker's reasons for excluding slavery and the coming of the Civil War are mostly persuasive, there is a flaw in the tone of the argument. By concentrating on matters away from the gathering national fire, his work becomes blander and slightly more complacent than is acceptable. The key to this is a small thing, but it reveals something that is unspoken in these pages. Frances Wright is described as "beautiful but outspoken" (p. 446). The little "but" is a red flag, a cover for a set of political and cultural assumptions that ought to be looked at very carefully. This book is full of pride in the American accomplishment without enough critical ballast. From reading it it is a little hard to imagine where Henry George came from. To be sure, the usual dis-

claimer is here: "To be sure, the female half of the population, and all but a very few Negroes, were excluded from voting" (p. 424). But this statement is not followed up; it is simply an introjection in wooden prose when compared to the hundreds of enthusiastic sentences that sing of the glory of the place.

My final hesitation derives from the interest of something Barker raises. Very early in the book he talks about the change in language toward the vernacular in the sixteenth century as reflecting "political restlessness" (p. 10). But he sees "little connection with politics" in the changes of language during and after the American Revolution (p. 380). He does not explain this different result, but he does acknowledge the centrality of language habits to intellectual history.

LOREN BARITZ

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PAGE SMITH. *Daughters of the Promised Land: Women in American History*. Boston: Little, Brown and Company. 1970. Pp. 392. \$8.95.

The other half of the American population is finally earning its fair share of historians' attention. Page Smith makes his contribution with what he calls "a generalizing book" about the role and importance of women since Plymouth Plantation (with glances as far back as ancient Egypt).

In many ways this is a gratifying study. It does indeed generalize, not only in chronological scope but also across the diversity of white middle-class women's concerns: the familiar matters of legal and economic opportunity; the less familiar but equally fervent religious activities; and the troubling realm of marriage, motherhood, and sexuality. At the same time his vivid anecdotes and biographical sketches give to his account the vitality born of the particular. The combination of wide research and graceful prose has produced a comprehensive narrative of women's rights and wrongs.

But every narrative is, of course, also an interpretation simply by the choice and arrangement of its evidence. To a large extent Smith interprets accurately and illuminatingly. Besides keeping a proper balance among

the many spheres of women's lives, he offers a thoughtful periodization of their history. Following the colonial era when women enjoyed virtual equality, the "Great Repression" placed them on the Victorian pedestal of the home and the moral double standard. In the latter nineteenth century the woman's rights movement flourished, climaxing in the suffrage and Prohibition amendments, but soon lapsed into the "Great Withdrawal" of the past fifty years.

This periodization makes good conceptual sense of the past. Unfortunately, Smith trespasses his own boundaries at several points. The chapter on "The New Prophets" extends from Freud to Helene Deutsch in the 1940s, while the chapter on "Home and Mother" spans 1830 to 1914. Such deviations would be excusable, considering the heterogeneity of his material, if they did not betray a more serious flaw. For Smith's preponderantly topical arrangement tends to blur not only chronology but analysis. The reader learns in successive chapters about marriage and sexuality, the woman's rights movement, missionaries, home and motherhood, the South and West, and finally prostitution—all nineteenth-century phenomena but none of them clearly linked by analysis. Moreover, in isolating the themes of marriage, home, and prostitution, Smith fragments the intellectual-moral Gestalt that they formed for Victorian Americans.

Conceptual flaws also occur at a deeper level. This book is, as Smith says, "full of theories and hypotheses," many of them provocative. For example, he suggests that Protestant fathers gave special encouragement to their daughters (hence the book's title), a relationship that helped to produce feminists. But he does not treat this important observation in a systematic way either by accounting for exceptions (such as Anna Howard Shaw and Charlotte Perkins Gilman) or by applying social-psychological research. Instead, he invites other scholars to test this and other hypotheses.

This unsystematic theorizing is tantalizing but tolerable until one finds that certain theories do not merely suggest but compete against one another. Smith stresses the motive force of "the Protestant passion" (rather than the Protestant ethic) behind reform movements like the Midwestern Women's Christian

Temperance Union (p. 175). But elsewhere he points to the growth of Northern urbanization and the middle class as the principal factors of female emancipation (p. 215), and still elsewhere he argues that industrial capitalism, based on the Protestant ethic, drew women from the home and fostered the basic attitudes upon which woman's rights leaders could build (pp. 282-86).

The theorizing goes entirely astray in the chapter on the nature of women, where Smith describes them as "private," "cosmopolitan," adept at routine tasks, "fearful liars," unwilling to make compromises, etc.—each trait the antithesis of men's. Such stereotypes contradict not only the evidence of the social sciences but the data of his own pages: Susan B. Anthony, for example, was hardly "private," while Margaret Sanger preferred melodrama to routine.

This lively and ambitious history of American women reaches for more than it can analytically grasp. Readers will be entertained and informed but ultimately, if they ask hard questions, frustrated.

PETER G. FILENE
University of North Carolina,
Chapel Hill

DOUGLAS WAITLEY. *Roads of Destiny: The Trails that Shaped a Nation*. Washington: Robert B. Luce; distrib. by David McKay Company, New York. 1970. Pp. xii, 13-319. \$7.95.

This account of twenty-two waterways, trails, and roads during the expansion of colonial America to the Mississippi River between 1635 and 1850 reflects the author's lifelong fascination with his subject. In preparation for his book he personally traveled the old roads to see them in their modern setting and to make comparisons with reports of wayfarers who used them in their heyday. He places heaviest emphasis on the French river roads between 1535 and 1750, the King's Highway from 1636 to 1774, and the road system during the American Revolutionary War. In contrast the Erie Canal, the National Road, and steamboats prior to 1850 receive only passing attention in the last fifty pages because, in his opinion, railroads cut their significance greatly after the 1850s. Waitley includes a number of excellent maps for the convenience of those who share

his interest, but he fails to provide an index, an equally important tool for guidebooks.

Professional historians and scholars in general may find the book irritating in many ways. The bibliography omits some very important monographs but includes textbooks in general American, colonial, and Western history, and not always the best of these. Although seemingly a book on the importance of transportation, *Roads of Destiny* reflects a great-man theory of history in stressing the work of individuals such as La Salle, William Johnson, and Sam Adams, and also provides much evidence for the prevalence of accident in history as demonstrated by the troubles that plagued travelers along the old roads. The focus further blurs because of the inclusion of interesting episodes from travelers' accounts, some of which would have benefited from greater attention to internal criticism. At times the author informs his reader as to the thoughts of travelers when they have left no such record, and he occasionally creates language that he considers suitable for them to have used at a dramatic moment. His lively prose occasionally becomes too much so, with wagons creaking on the National Road, slave chains clanking on the Federal Road, and paddle wheels churning on the Ohio-Mississippi waterway—all in one sentence. The numerous misspellings should have been corrected by the editor.

LEWIS ATHERTON
University of Missouri,
Columbia

LUTHER TWEETEN. *Foundations of Farm Policy*. Lincoln: University of Nebraska Press. 1970. Pp. xi, 537. \$9.50.

Although the topical approach of the social scientist does not always lend itself to chronological exposition, this splendid study has much to offer in the way of insights. Masterfully synthesizing a panorama of historical events, the author highlights the political, economic, and social forces that contributed most to the development of farm policy in the United States.

Luther Tweeten argues strongly that "current economic environment and current policies [dealing] with farm problems . . . have deep roots in the technology and attitudes of

earlier decades." He contends furthermore that contemporary issues relating to overproduction, rural poverty, and the disappearance of family farms are all interrelated. Relentless progress in technology, such as mechanical invention, scientifically produced chemicals, and hybrid seeds, steadily drove down the number of farmers needed on the land, while at the same time food production increased by leaps and bounds. This factor more than any adherence to an abstract ideology, asserts Tweeten, converted the rural populace from its staunch individualism to a belief in collective action.

Analyzed thoroughly is the transition in agrarian attitudes that made for general acceptance of government assistance. The Protestant work ethic was sufficiently modified in the twentieth century to justify the banding together of farmers into national organizations or smaller regional groups representing specific commodities. Emulating labor unions, farmers also sought and obtained minimum income guarantees—albeit in the form of price supports. Tweeten concludes correctly that such action proved far more effective than earlier third-party crusades.

This study has value for urban-oriented historians who discount too easily the difficulties encountered by true tillers of the soil. To earn a living from the land, dirt farmers must first contend with rapidly rising operating expenses and then face such obstacles as high real-estate taxes, adverse weather, unpredictable markets, fluctuating credit conditions, crop-destroying insects, plant diseases, and finally the prospect of price-depressing surpluses. Often the margin between profit and economic ruin for small units is provided by the subsidies received for participation in federally sponsored farm programs. If Tweeten's book proves anything, it establishes beyond a doubt that solutions to farm problems will not be simple. Merely to inquire "Why pay farmers for not producing?" answers nothing. This would be tantamount to asking why an unskilled worker should be paid a minimum wage he does not really earn or posing the question as to why a welfare recipient should receive a dole for being idle. In these areas obviously concerns of national welfare and social justice enter the picture—as they do with agriculture.

The only flaw in this work, that is from a historian's viewpoint, is the author's tendency to play down the significant roles of certain key farm leaders. A dominant figure such as Henry A. Wallace, for instance, receives only one mention and that unrelated to the Triple-A program of the New Deal. Aside from this minor criticism, Tweeten has done a really excellent job in tracing and analyzing major trends in farm policy from the nation's birth to the present. His ability to write clearly and succinctly and to use quantitative data in an understandable manner makes this a highly readable book. Its scope and depth will assure wide usage among those interested in agricultural history.

EDWARD L. SCHAPSMEIER
Illinois State University

RAYMOND PHINEAS STEARNS. *Science in the British Colonies of America*. Urbana: University of Illinois Press. 1970. Pp. xx, 760. \$20.00.

Raymond Stearns's first essay in the history of colonial science was in Belgium, in page proof for *Osiris*, when the German Blitzkrieg swept through the Lowlands. In June 1940 George Sarton wrote defiantly from Massachusetts that all the articles would be published, "perhaps sooner than we think." Stearns's article, "Colonial Fellows of the Royal Society of London, 1661–1788," was printed, but not until 1946 and then in the *William and Mary Quarterly*. He thus stepped out virtually alone on a path that brought him to this work of encyclopedic scholarship, a work in every way worthy of Sarton's memory.

Science in the British Colonies of America is a magisterial work, beautifully written. Its perspective is that of the Royal Society. From the vantage point of the Society's archives and the resources of the British Museum, Stearns was able to view his subject as a whole. This is perhaps the only stance from which a scholar can see beyond regional and topical prominences in the colonial panorama. From this position the natural sciences of New World flora and fauna inevitably loom larger and are dealt with in greater detail than are astronomy or the experimental sciences. Stearns gives large place to the great published editions of natural science. But he has also emphasized consist-

ently that his history of science is a history of men at work in the sciences. Biographical sketches abound and are most informative. Of course, not all areas of science are described with equal finality. I myself think of New England in the last quarter of the seventeenth century; there may be others. But even so I have no quarrel with the relative emphasis and proportions of the work. It is unsurpassed for balanced organization and wide erudition.

During most of the period colonial science was directed by the activities of the Royal Society. By mid-eighteenth century, however, there was, Stearns writes, an "emerging American science": "For the American colonies, at least, the development of science was a *social* achievement," which developed from "a subtle union of social forces originating in economic, demographic, and urban growth." An "American science" comes naturally, almost compellingly, as the climax of a development so closely parallel with the movement of colonial society toward maturity and independence. But it is a dubious interpretation. Following the model suggested by George Basalla ("The Spread of Western Science," *Science*, 156 [1967]: 611-22) American science before the Revolution was still colonial science. Such a difference of opinion, however, does little to vitiate the achievement of a remarkable work of scholarship.

MICHAEL G. HALL
University of Texas,
Austin

BERT ANSON. *The Miami Indians*. (The Civilization of the American Indian Series, Volume 103.) Norman: University of Oklahoma Press. 1970. Pp. xvii, 329. \$8.95.

The French first encountered the Miami Indians in present-day Wisconsin, but by 1725 the semiagricultural tribe was firmly established in the fertile Wabash-Maumee river valley of Indiana, the area the tribe considers its historic homeland. Never a numerous people, perhaps two thousand at the close of the French period, the tribe exerted an influence far beyond its size because of its strategic geographic position, brilliant leadership, and uncanny sophistication in coping with the European intruders. By the close of the American Revolution, as the author points out, the

Miami had "emerged as a bulwark of Indian resistance against white expansion north of the Ohio River" (p. 95). Under Little Turtle the tribe enjoyed its finest hour, humiliating two invading American armies before meeting defeat at Fallen Timbers in 1794. Thereafter the story of the Miami is no different from that of most of the other tribes: they watched helplessly as their homeland melted away. Surprisingly, the Miami held out until the mid-1840s before removing to the area that is now Oklahoma. Even then only half of the tribe left Indiana; many of the mixed bloods were allowed to remain on private reserves and a core of acculturated Miami are still in the state.

Anson tells a simple, factual story. His book's greatest strength lies in the account of Miami emigration and new life in the West, based on information provided by papers in the custody of tribal leaders and related records in Oklahoma. The ethnohistorical overview of aboriginal Miami life and the account of the relations of the tribe with the federal government prior to removal are less satisfactory.

Although Anson has provided the first general history of this relatively obscure tribe, which is a useful addition to *The Civilization of the American Indian* series, his book is not without flaws. With a great deal of basic source material readily available on microfilm or in such standard works as the *Territorial Papers of the United States*, it is disappointing to find so much reliance upon secondary works. And the book is marred by errors in transcribing quotations, misplaced captions, garbled footnotes, and an inadequate index.

HERMAN J. VIOLA
The National Archives

THOMAS O'BRIEN HANLEY. *Charles Carroll of Carrollton: The Making of a Revolutionary Gentleman*. Washington: Catholic University of America Press. 1970. Pp. x, 293. Cloth \$12.95, paper \$9.95.

Biography is a tough business. Once the biographer has completed sifting the manuscript remains, he frequently comes either to love or hate his subject. Having spent so much time in his character's company, any sense of dispassionate analysis becomes inordinately dif-

ficult. Such is the case with this compassionate study of an engaging man, Charles Carroll of Carrollton. Like those marvelous studies (too often dismissed as of little value) produced in the era between Bancroft and McMaster, Hanley's work is full of useful information and yet is characterized by an overt canonization of Carroll into blinding heroic dimensions.

Carroll had a long life. Born in 1737 he lived until 1832 and enjoyed the distinction of being the last signer of the Declaration of Independence to die. Hanley's study concludes in 1773 when Carroll was thirty-six and just emerging as a leading "revolutionary gentleman"—a man who wanted dramatic change but no violence. He was by then the product of many factors. As a Catholic who had lived in discriminatory societies both in Maryland and England, he resented those restrictions denying him full participation in the broader secular community. As the beneficiary of rich experiences both educational and social, including some sixteen years in Europe while attending Jesuit-manned institutions in France and the English Inns of Court, he had become a gentleman and scholar. Back home after his studies abroad he continued to work hard, to study, and to pursue the correct social pleasures. According to Hanley, Carroll's conduct always merited admiration as did his thought, which was primarily a combination of Montesquieu and a cyclical theory of history. Believing the British Empire to be eroding under the corrupting influences of selfish factionalism, Carroll concluded that true republican virtues could only flourish in the colonies if all forms of imperial influence were suppressed and replaced by more responsible institutions. Specifically Carroll envisioned a society governed principally by a powerful upper house, fashioned in the Roman senatorial mold, where only the better sort would be allowed to sit, isolated from the corrupting interests affecting men of lesser quality.

All these many details are creatively reconstructed. Though the prose sometimes becomes too grandiloquent, many of the chapters, particularly those concerned with the young man's educational and social experiences, are delightful. Hanley's analysis of Carroll's motivations

must nevertheless be questioned. Too often Enlightenment ideals are portrayed as the sole basis of action because of their expression. Thus Carroll, the gentry idealist, assumes a public role primarily because he wishes to reform his society's institutions to make them "the servant of the people and the guardian of their liberty." But Carroll's ideal senate, for all its rhetorical devices, was not designed to express the will of the people. Rather his class commitment, which was as strong as his attachment to Enlightenment ideals, made him anxious to control the popular passions that existed in a society characterized by stratification and animosity. The Carroll fortune in the years just prior to the Revolution equaled close to £90,000. To keep it secure was the real business of a gentleman confronting a potentially revolutionary situation—or so I believe.

RONALD HOFFMAN

University of Maryland

SIR EGERTON LEIGH and ARTHUR LEE. *The Nature of Colony Constitutions: Two Pamphlets on the Wilkes Fund Controversy in South Carolina*. Edited and with an introduction by JACK P. GREENE. (Tricentennial Edition, Number 1.) Columbia: University of South Carolina Press for the South Carolina Tricentennial Commission. 1970. Pp. 232. \$7.95.

In 1769 the South Carolina House of Assembly sent £1,500 sterling for the London hero John Wilkes, who had defied Parliament, caused innumerable riots, and repeatedly insulted an angry king. Lieutenant Governor William Bull signed no warrant for the grant. He referred his embarrassing problem to the American secretary, Lord Hillsborough, and the governor received orders from Whitehall to stop a practice tolerated in the colony since 1750. Encouraged, the governor and the council fought the Commons to a stalemate, but tax bills were denied, legislation was ignored, royal government was paralyzed, and the Commons even questioned the legitimacy of the council as an upper house of the legislature.

Jack P. Greene has called the Wilkes Fund controversy "the bridge to revolution in South Carolina." In an extended introduction to this volume he recapitulates his earlier argument

and then suggests that the two pamphlets, here edited and reprinted for the first time since 1774, could help to bridge a gap in our understanding. They represent polarities on the nature of colony constitutions and point up the ambiguous relationship between the colonists and the Crown.

One pamphleteer argued mainly from eighteenth-century reason, the other chiefly from custom and precedent. On the side of the Crown was Sir Egerton Leigh, attorney general, surveyor general, president of the South Carolina Council, and by his own admission a "downright placeman" for twenty years. Leigh maintained with wit and reason that the South Carolina constitution derived from the will of the king and that the colonists could not alter it. On the side of local precedent and the Commons were Henry Laurens and Ralph Izard, Jr., who collected data and engaged Arthur Lee of Virginia to write the rebuttal. Lee, a law student at the Middle Temple and agent for Massachusetts in London, had paid his patriotic dues with perhaps the busiest pen in the colonial arsenal. He would have taken strong exception to only one of Professor Greene's introductory remarks, that John Wilkes harbored "a notorious dislike for Americans." Lee was Wilkes' right-hand man and a past secretary of the Society for the Supporters of the Bill of Rights, the patriot's political organization in London.

These two skillfully edited pamphlets will not clear up the dispute over the nature of colony constitutions. Nor will they define once and for all the precise nature of the Crown's authority. But they should spark new interest in a relationship far too obscure to most of us.

A. R. RIGGS

McGill University

LUELLA J. HALL. *The United States and Morocco, 1776-1956*. Metuchen, N. J.: Scarecrow Press, 1971. Pp. x, 11-1114. \$25.00.

A massive tome, 1,114 pages set in 9-point type, fifteen chapters with an average of 170 notes per chapter, with glossary and index! The author intended a long overdue beginning of "the task of historiography" of United States-Moroccan diplomatic relations, cogently pointing out that the relations of United States with

Morocco have been more extensive than those with any other minor country, a fact ignored by eminent scholars like Nevill Barbour in his discussion of the Algeciras Conference. United States-Moroccan relations date, importantly, from the very birth of the United States, and they engaged the attention of Jefferson, Franklin, John Adams, John Quincy Adams, Seward, Lincoln, the two Roosevelts, Truman, Eisenhower, and John F. Kennedy.

The book falls short of its lofty goal. It does bring together in one volume a summary of a large number of secondary sources, many of them rather rare, but it lacks a bibliography, and the notes do not provide the place or date of publication or the publisher. Short titles are used after the initial citation, and it is difficult to wade back through perhaps one thousand notes to find the full citation. In addition the book digresses at length into some worldwide aspects of United States diplomacy of little pertinence to Morocco.

The secondary sources, particularly in the modern period, are not well chosen. They are for the most part contemporary English-language journalism, polemical in nature. Moroccan sources are conspicuous by their absence. Significant French-language primary sources like the memoirs of Marshal Lyautey, Marshal Juin, and several other residents-general in Morocco are ignored in favor of writers like Kenneth Pendar and Rom Landau. Pendar was a United States vice-consul in North Africa during World War II, and his *Adventures in Diplomacy* (1945) is a self-serving, anti-de Gaulle defense of the policy of the United States toward Vichy France. Landau is a Moroccan court historian, perhaps more a Moroccan nationalist than Ahmed Balafrej, the titular head of the Moroccan Istiqlal party. To use such sources without an interpretive warning is to perpetuate myths that ought not to have a place in diplomatic history.

The author made substantial use of consular dispatches, and one might have hoped to see some of the diplomatic myths laid to rest—for example, the one that President Franklin D. Roosevelt "pledged American support for the independence of Morocco in his interview with the Sultan in Casablanca" on January 22, 1943 (pp. 1065, 1002); the evidence that

Roosevelt favored no change in the French protectorate in Morocco is contained in the National Archives file 881.00, which the author used extensively.

Two chapters (less than two hundred pages) are devoted to United States relations with Morocco after the Allied landings on November 8, 1942, a period in which the involvement of the United States in Morocco surpassed the total of all previous involvements, yet the most pervasive influence, that of the United States military presence, passes almost unremarked. The Franco-American agreements of December 22, 1950, for the construction of the United States Air Force bases in Morocco and the expanded use of the United States Navy base at Port Lyautey are not even identified, and the whole topic of the re-establishment of a strong United States military presence in Morocco pursuant to those agreements, a proximate cause of the Moroccan revolution, is treated in a single paragraph (pp. 1040-41).

The deficiencies, however, must not be allowed to obscure the positive contributions this book makes to the scholarship on Morocco. After all, one author can hardly be expected in one volume to clean up the historiography of half a century. *The United States and Morocco, 1776-1956* contains a vast store of factual information. The material gleaned from the consular dispatches alone would justify an excellent, normal-sized volume. *The United States and Morocco* will stand as a valuable source for further study.

LEON B. BLAIR

University of Texas, Arlington

JAMES THOMAS FLEXNER. *George Washington and the New Nation (1783-1793)*. Boston: Little, Brown and Company. 1970. Pp. xi, 466. \$12.50.

This book is not based on intensive research in the vast mass of materials for the years 1783-93, and it does not treat in depth the most significant issues of the times. It does contain, however, some valuable insights and ideas. The importance of Washington as a national symbol and of the precedents he established as the first president under the new Constitution are emphasized, and they are all too often forgot-

ten. The assertion that Washington was an "overpowering genius" should call for at least some mild debate. Admirers of Jefferson should be intrigued by the description of Jefferson's "sadism" in his meetings with Washington and by the statement that "he acquiesced in a new method for torturing his chief" in not preventing Philip Freneau's newspaper attacks on Washington.

Flexner writes with sympathy, and above all with imagination, a quality much needed in dealing with a man as aloof and self-contained as Washington. Washington wrote thousands of letters and kept diaries, but most of the letters are cool and formal, and the diary entries are limited to bare facts. They seldom reveal the inner man or his feelings, particularly when compared to the intensely human and often fervid letters and diaries of a man like John Adams. Occasionally a novel like Howard Fast's *The Unvanquished* (1942) has presented a plausible account of what the inner Washington might have been like, but most biographers have failed when they have tried. Flexner has a novelist's imagination, although it sometimes leads him onto shaky ground, as in his account of Washington's thinking about a second marriage and children if his wife should die. The letters cited really do not support this story, fascinating though it is.

The best part of the book is the account of the years at Mount Vernon between 1783 and 1789. Washington was uneasy in the "retirement" he had longed for, and desperately short of money. But he was unable or unwilling to reduce his rather splendid scale of living, and he had to borrow money to go to New York to be inaugurated as president. He offered to serve for expenses as he had in 1775 when elected commander in chief. Congress ignored the offer and voted him an annual salary of \$25,000. Flexner does not mention the recent irreverent book about Washington's "expense account living" during the Revolution, but he does mention that it cost Washington about \$5,000 a year in addition to his salary to maintain himself as president.

Flexner's account of the Washington household and social life is fascinating and worthwhile. The discussion of the political issues

and rising partisanship during Washington's first term is inadequate, even for those areas in which he was intimately involved such as the fight over the location of the national capital. Thus while the book is useful and interesting as a study of Washington as a man, the scholar will have to turn elsewhere for most important ideas and information about the years covered by the book.

MERRILL JENSEN
*University of Wisconsin,
 Madison*

CHARLES G. CLARKE. *The Men of the Lewis and Clark Expedition: A Biographical Roster of the Fifty-one Members and a Composite Diary of Their Activities from All Known Sources.* (Western Frontiersmen Series, Number 14.) Glendale, Calif.: Arthur H. Clark Company. 1970. Pp. 351. \$12.50.

There is a mystique about the Lewis and Clark Expedition. It has something to do with the magnificent new country that was crossed by the Corps of Discovery. It involves the intriguing success of a dual leadership, something that has no counterpart in all of the history of exploration. Some of it comes from the journal entries describing sickness and peril and from the miracle of an expedition that lost just one member by death. There is the constant tension of the meetings with Stone Age tribesmen. And there is the dancing, the wenching, the hunting, the *joie de vivre* of thirty to fifty men, one young woman, and a Newfoundland dog, all of them engaged as a unit in a great adventure. Indeed, the world has no happier tale of exploration than the story of the Lewis and Clark Expedition. Little wonder, then, that the books about it keep pouring forth.

Mr. Clarke's volume is a minor but still a positive addition to the growing library of Lewis and Clark material. He accomplishes his restricted goal with care and discrimination. The author presents brief biographies of the fifty-one men who he believes started up the Missouri in May 1804; he also includes those who joined after the expedition was under way. It will be noticed that fifty-one members are five or six more than are usually credited to the expedition, but Clarke appears to have done his genealogical detective work well, and

his evidence substantiating this number is convincing.

Having accomplished this, Clarke then offers a component "personal diary," a day-by-day record of the journey based upon all the known materials. He has modernized the spelling, maintained continuity with bracketed phrases of his own composition, and eliminated all extraneous material. What emerges is, in essence, the chronicle of a hunting expedition, with the entries almost entirely concerned with the personnel. His aim is to reveal the personalities of the members as they react to the trials and tribulations of life on the trail.

For sheer interest, albeit in the narrow framework of men on the move, Clarke's "diary" is an excellent manipulation of Lewis and Clark materials. As for his overall aim, he succeeds as well as can be expected. All the members of the expedition appear somewhat clearer, a little less shadowy, which makes them more intriguing than ever. Several illustrations and a map round out this volume.

RICHARD A. BARTLETT
Florida State University

FRANCES LEA MCCURDY. *Stump, Bar, and Pulpit: Speechmaking on the Missouri Frontier.* Columbia: University of Missouri Press. 1969. Pp. xiii, 218. \$7.50.

WALDO W. BRADEN, editor, with the assistance of J. JEFFERY AUER and BERT E. BRADLEY. *Oratory in the Old South, 1828-1860.* (Prepared under the auspices of the Speech Association of America.) Baton Rouge: Louisiana State University Press. 1970. Pp. 311. \$10.00.

Two of the eight chapters in Frances McCurdy's book set the stage for what follows in her discussions of "The Pioneer Missourian" and the rhetorical training of young Missourians. Two other chapters have to do with developments at the bar and in the pulpit. Three of the remaining four are chiefly political, and "Rhetoric: Reflection of the Frontier" completes the work.

The contribution edited by Braden consists of nine rather closely related essays by ten people, most of them professors of speech. Beginning with the nullifiers and antinullifiers of the 1820s and 1830s and continuing down through "The Southern Unionists, 1850-1860,"

nearly all the studies are political-rhetorical in orientation. The book contains little about church or courtroom oratory. One chapter is devoted to "Speaking in Southern Commercial Conventions"; another offers a provocative appraisal of a single major figure, John C. Calhoun, with concentration on his defense of slavery. In addition to editing the essays of his associates, Braden opens *Oratory in the Old South* with a first-rate introduction.

There is abundant evidence in each of these books that the blue pencil has often been skillfully wielded. Inadequate indexing mars the McCurdy product in the case of Uriel Wright, for instance, and also in the omission of Samuel A. Foot and Henry S. Foote. The last name of Foot (of the Foot Resolution) and the first name of Seargent Smith Prentiss are among those misspelled in the text. On the other hand, Professor McCurdy comes forward with discerning comments on the attitudes of Missourians toward the speaking of John Quincy Adams, Thomas Hart Benton, and other prominent as well as minor figures.

Outstanding in *Oratory in the Old South*, both substantively and stylistically, is Robert G. Gunderson's "The Southern Whigs." Lindsey S. Perkins, in his essay "The Moderate Democrats, 1830-1860," emphasizes the careers of Alexander H. Stephens and Robert Toombs prior to 1852. I consider this emphasis disconcerting, even though Perkins explains, at least to his own satisfaction, the inclusion of these Whig leaders. Good as it is, "John C. Calhoun's Rhetorical Method in Defense of Slavery" might have been stronger if Bert E. Bradley and Jerry L. Tarver had compared their conclusions with those of historians Gerald M. Capers and Charles M. Wiltse.

Of decided interest to older scholars, and containing much long-fugitive material integrated with the more familiar, the Braden and McCurdy volumes may be of greatest value in what they will convey to nonhistorians and fledgling historians concerning public address during parts of the 1810-60 period. Amateurs and graduate students alike can benefit appreciably and quickly by scrutinizing these two creditable books. Here are no perpetuations of stereotypes, no mere reproductions of oratorical myths. Both *Oratory in the Old South* and *Stump, Bar, and Pulpit* are the results of probing analyses, produced by experts in speech

patterns who not only report what representative men said but also offer informed interpretations.

HOLMAN HAMILTON
University of Kentucky

ARTHUR H. DEROSIER, JR. *The Removal of the Choctaw Indians*. Knoxville: University of Tennessee Press. 1970. Pp. xii, 208. \$7.50.

THURMAN WILKINS. *Cherokee Tragedy: The Story of the Ridge Family and the Decimation of a People*. [New York:] Macmillan Company. 1970. Pp. x, 398. \$10.00.

The removal policy, particularly as applied to the Five Civilized Tribes, continues to be one of the most popular themes for writers of Indian history. Indeed, the Cherokee "Trail of Tears" has become a generic term for the ordeal of those Indians east of the Mississippi who were forced to move to new homes west of the river. In part this is a reflection of the abundance of materials pertaining to these tribes. In addition to the usual government documents we have the correspondence of missionaries active among the Choctaws and Cherokees. Both tribes had a relatively large number of educated mixed bloods, and the Cherokees published their own newspaper during the removal crisis. They also commanded the support of a political faction interested as much in inflicting a defeat on President Jackson as furthering the welfare of the Indians. DeRosier and Wilkins both have exploited fully these ample sources.

The authors have emphasized the events leading to removal of the Choctaws and Cherokees. Shortly after the War of 1812, in which, ironically, they contributed to the American war effort, both tribes began to be pressured to exchange their ancestral homes for new lands in what is now Oklahoma. The demands of white land seekers supplied the motivation; the rationalization was that only by withdrawing out of reach of the white settlements could the Indians gain the time to civilize themselves and escape extinction as a people.

DeRosier's story is told in an unadorned but adequate prose. It expands on the earlier work on Choctaw removal done by Grant Foreman, Angie Debo, and others and clearly depicts the American pressure and the tribal factionalism that reduced the effectiveness of Choctaw resistance. Unfortunately, DeRosier

has attempted to fit his story of Choctaw removal into a thesis that sets up John C. Calhoun in opposition to Andrew Jackson. Calhoun emerges as the exponent of gradualism, who believed that the Indians should be removed only after they had been educated, whereas Jackson is the hard-liner who will tolerate no Indian delay. The difficulty with this thesis is that Calhoun left the War Department in 1820, before any Choctaws had been removed and ten years before the negotiation of the Treaty of Dancing Rabbit Creek that produced the major Choctaw migrations. One suspects that if Calhoun had been secretary of war in 1830, and subject to the increased agitation for Indian removal the growth of white population in Mississippi had produced, the treaty negotiations and Choctaw removal would have followed essentially the same course.

Cherokee Tragedy was a pleasant surprise. Instead of still another warmed-over version of a familiar story—the Cherokees have had more than their fair share of historians—Wilkins, an English professor, provides the best researched and most readable account to date of this period of Cherokee history. Even more surprising is the selection of his heroes, the Ridges. Major Ridge and his son John were leaders of the Cherokee faction that considered removal the lesser of the evils facing their people. They were subsequently murdered by Cherokees who invoked a tribal law calling for the death penalty for Cherokee leaders who sold tribal lands without tribal approval. Making the Ridges the heroes, as opposed to John Ross, who led the resistance to Georgia and Jackson, is almost as heretical in the 1970s as lamenting the fate of George A. Custer. Nevertheless, Wilkins makes a good case for the Ridges' being realistic enough to recognize that resistance was futile and could only aggravate the plight of the Cherokees.

WILLIAM T. HAGAN
State University of New York,
Fredonia

JACOB LANDY. *The Architecture of Minard Lafever*. New York: Columbia University Press. 1970. Pp. xii, 313. \$17.50.

Minard Lafever practiced architecture in New York City from the late 1820s until his death in 1854. He published five books (one post-

humously) primarily for fellow architects. His influence on building in young America was considerable and, for several churches he designed and built in Brooklyn, his reputation is both justified and sound.

In his lifetime Lafever married three times, was twice a widower, fathered eight children, and taught himself to be an architect. He was probably trained in carpentry at a time when the distinctions between builder and architect were not as clear as they are today. But few of his personal or business records survive. The man and his work are imprecisely perceived; even a list of his buildings is incomplete and contains many "attributions." When the first volumes of *The Dictionary of American Biography* were in preparation, Talbot Hamlin wrote a sketch of Lafever. Through an error the sketch was neglected and then appeared in the first supplemental volume. All this seems to suggest that Minard Lafever was resting in peace and should not be disturbed.

In calmer days such might have been the case, but not in the Searing Sixties: then, when a doctoral candidate asked an established master for a dissertation topic and got it, what could he do? And, when the candidate finished his research and the master recommended publication to a neighborly university press, could a book be but close behind?

Jacob Landy researched and wrote his dissertation—"The Architecture of Minard Lafever in Relation to the New York Scene from 1825-1855"—at the suggestion of Henry-Russell Hitchcock. Columbia University Press now offers the efforts of that labor as a book. Of the book it must be said that the format is unpleasant, the notations are at the end of the text, the photographs are badly composed and not conveniently placed in the text they are supposed to illustrate, and the entire production is not up to the standard set by Hugh Morrison's *Louis Sullivan* thirty-six years ago. It is not worth the price asked.

DAVID H. CROOK
Dalhousie University

ANNE FIROR SCOTT. *The Southern Lady: From Pedestal to Politics, 1830-1930*. Chicago: University of Chicago Press. 1970. Pp. xv, 247. \$5.95.

This book provides ample evidence, if any is

needed, that intelligent and imaginative women's history is at last being written. In describing the changing situation and outlook of the South's middle- and upper-class white women (or "ladies") between the slavery era and the Great Depression, Anne Firor Scott makes a series of important points. First of all, she probes the difference between the image and the reality of a woman's life in the antebellum South and finds an important undercurrent of discontent with the patriarchal institutions of a slave-holding society. Her discoveries suggest that the myth of "the queen of the home"—that joyfully submissive paragon of domestic virtue to whom Southern gentlemen paid sentimental homage—will have to go the way of the comparable legend of the indulgently treated, docile, and contented slave. Next she describes how the Civil War emancipated many Southern women from male-imposed domesticity by impoverishing the South and killing or demoralizing a substantial proportion of the male providers, thereby forcing widows, single ladies, and wives whose husbands could not adapt to the new order to take up a variety of occupations and professions hitherto generally closed to women. Paralleling this increase in the extent and diversity of women's work outside the home was a gradual growth in independent organizational activities among women. The formation of female missionary societies and women's clubs was followed by growing public involvement of women in various reform efforts, especially temperance, the crusade against industrial exploitation of women and children, the campaign for improved race relations, and, of course, women's suffrage. Ultimately, according to Professor Scott, women were able to play a significant role in Southern Progressive reform during the first three decades of the twentieth century.

The aspect of the book most likely to be controversial is its treatment of the implications of the Southern women's suffrage movement. Disagreeing with some recent interpretations, Professor Scott argues that the movement was neither conservatively narrow in its reform goals nor motivated to any significant extent by a racist desire to counteract the black vote. In seeking to place the Southern suffragists and, by implication, the whole twentieth-

century women's suffrage movement in a more favorable light, she has, at the very least, managed to reopen the question of what gave impetus to the campaign. Professor Scott is probably right in her contention that suffragists were basically concerned about the place of women and not about the place of blacks. Yet a suspicion persists that the active and articulate women of the New South were more infected by racism than this account acknowledges. Although she records women's activities on behalf of moderate interracial reform, she overlooks evidence suggesting that some champions of women's rights contributed to the climate of race hate that coexisted with Southern Progressivism. Rebecca Felton, a prominent Georgia feminist, is described in the book as a leader of the Women's Christian Temperance Union and as an enemy of the convict lease system, but nowhere do we find any hint that she was also a conspicuous racial extremist who predicted black extinction, advocated disfranchisement, and justified lynching. How many Rebecca Feltons were there? Not many perhaps, but a more detailed study of the racial views of Southern feminists is obviously required before we can convincingly absolve the women's movements of the Jim Crow era from the suspicion of active complicity in the triumph of racial oppression.

GEORGE M. FREDERICKSON
Northwestern University

STEVEN A. CHANNING. *Crisis of Fear: Secession in South Carolina*. New York: Simon and Schuster. 1970. Pp. 315. \$7.95.

A generation ago, when the Negro was still pretty much an "invisible man" to most American historians, they could practically ignore him in discussing such matters as the secession movement. Thus in *The Coming of the Civil War* (1942) Avery Craven disposed of slavery as a mere symbol of other and more fundamental issues. But as historians view the antebellum period in the reflected light of the 1960s, they see looming large the shadow of the black man, if not the black man himself. William W. Freehling, for example, in *Prelude to Civil War* (1966) has looked beneath the tariff controversy to discover a slave threat as the basic concern of South Carolina nullifi-

cationists in 1832–33. In *Crisis of Fear* Steven A. Channing reveals the same kind of motivation for the South Carolina secessionists of 1860.

“Secession,” Channing says, “was the product of logical reasoning within a framework of irrational perception.” The secessionist politicians perceived “the will of the Northern people to destroy slavery” in the abolitionist agitation, the John Brown raid, the Republican endorsement of Helper’s *Impending Crisis*, and the election of Lincoln, a portent of his party’s early control of the entire federal government. In submission to Republican rule the secessionists foresaw economic and social ruin—not only the loss of billions of dollars in slave property but also race war or else the acceptance of Negro equality and, in the words of the Baptist preacher James Furman, “the marriage of your daughters to black husbands.” Seeing safety only in disunion, the radicals welcomed the Republican victory as an event they might use to precipitate secession. They and those who followed them were responding far more to concrete racist fears than to vague sectional grievances. Slavery was the essence, not the symbol.

In presenting his argument Channing gives a detailed account of South Carolina politics in 1859 and 1860. He has gathered his facts from a wide variety of manuscript and newspaper sources, and he marshals them ably in a well-organized and on the whole well-written book, one worthy of the Allan Nevins Prize the Society of American Historians has awarded it. The publishers have put the copious footnotes where footnotes belong but have provided poor copyediting and an inadequate index.

RICHARD H. CURRENT

University of North Carolina,
Greensboro

ALVY L. KING. *Louis T. Wigfall: Southern Fire-Eater*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1970. Pp. ix. 259. \$10.00.

Louis Trezevant Wigfall grew to manhood in the fanatical proslavery plantation society of South Carolina during and after the nullification crisis. By 1844, when he was twenty-eight, Wigfall was a confirmed secessionist who

had, in King’s opinion, absorbed all the worst characteristics of the Palmetto State’s plantation elite. He was a strutting cock-of-the-walk bundle of ambition, arrogant pride, tender feeling, dogmatic conviction, and extreme self-righteousness—all topped by a hot temper, reckless courage, and a loose tongue. His Union-hating, Southern-rights, white-racist vocabulary was limitless.

Having ruined himself as a lawyer and politician in South Carolina by killing a popular young man in a duel, Wigfall moved to Texas in 1846 seeking a new start. He rose to power as a leader of the Southern-rights wing of the state’s Democratic party, winning election to the United States Senate in 1859 and gaining thereby the power to help destroy the Union. His chief significance in national history lies in the virulent, unyielding stand he took in the Senate in behalf of Southern rights and against any possible compromise.

Wigfall’s career as a Confederate senator, to which half the book is devoted, was significant in two important respects. First, he was an influential leader and a zealous advocate of strong military policy—all of which endeared him to the Confederate brass but made for trouble in the ranks and on the home front. Second, he became involved in a quarrel with President Davis over military policy that was discreditable to both men and disastrous for their common cause.

For two-hundred-odd pages King presents Wigfall as a perfectly understandable fire-eater. He then weakens his otherwise valuable study by indulging in a perplexing bit of retrospective psychoanalysis and an equally puzzling piece of moral determinism. He thinks his senator may have been a paranoid compelled by his subconscious fears and doubts to attack the Union, oppose President Davis, and create discord in the Confederacy.

But this hardly matters, for, wandering off into the supernatural, King argues that the South, cursed by the sin of slavery and a fatal attachment to states’ rights, was doomed to lose against the overwhelming moral forces mustered in behalf of the Union. Whether or not this puts his paranoid senator, the destroyer of the Confederacy, on the side of the angels we are not told. Since it is unlikely that the neo-abolitionists will now claim Wigfall or that

the United Daughters of the Confederacy will ban him as a hero, the profession will likely go on viewing him as an understandable though tragic figure of his age and ascribing him more humanness than King is willing to concede.

WALTER L. BROWN
University of Arkansas

CHARLES PRESTON FISHBAUGH. *From Paddle Wheels to Propellers: The Howard Ship Yards of Jeffersonville in the Story of Steam Navigation on the Western Rivers*. Indianapolis: Indiana Historical Society. 1970. Pp. xiii, 240. \$6.50.

From Paddle Wheels to Propellers is a history of the Howard Ship Yards and Dock Company of Jeffersonville, Indiana. To a considerable extent the book reflects the story of steam navigation on the Western rivers. The Howard Ship Yards was a family enterprise established by James Howard in 1834 and carried on by four generations of the Howard family. It was a relatively successful enterprise until the 1930s when the depression caused financial difficulties. In 1942 the navy acquired the Howard Ship Yards and made additions. After the end of World War II the navy sold the improved yards to Jeffboat, Incorporated, which has continued to build towboats, barges, and ocean-going ships.

It would not have been possible to write this book if the four generations of Howards had not preserved most of the correspondence, ledgers, and other records of the family enterprise. The papers of the Howard Ship Yards and Dock Company, now owned by the Lilly Library, Indiana University, Bloomington, consists of more than 265,000 items.

The book is well illustrated with photographs, tables, figures, and appendixes. The forty photographs are mostly of steamships. Twelve tables present statistics about the Howard Company and freight traffic on the Western rivers, and four figures present statistics on steamboat construction on them. Appendix A gives the output of the Howard Ship Yards by years from 1834 to 1940. Appendix B gives the number and gross tonnage of steamboats constructed on the Western rivers from 1811 to 1937.

A copy of this book should be bought by all public and college libraries. The reasonable

price will enable transportation buffs to acquire copies for their private collections.

JOHN H. KRENKEL
Arizona State University

NORMAN D. BROWN. *Daniel Webster and the Politics of Availability*. Athens: University of Georgia Press. 1969. Pp. vii, 184. \$6.50.

Daniel Webster, like Henry Clay and John C. Calhoun, was fated never to attain the presidency, despite the fact that like his mighty contemporaries he hungered desperately for the office. In this short and clearly written book Norman D. Brown focuses on the years between the Webster-Hayne debate and the election of 1836, the period in which the "god-like" Daniel was bitten most severely by the presidential bug. Mr. Brown's unpretentious volume tells more fully than it has been told before the dramatic story of the amazing near-rapprochement between Webster and President Jackson, Webster's falling out and later partial reconciliation with Clay and Nicholas Biddle, and the rise and fall of Webster's expectations prior to 1836. Particularly effective is Brown's sure-handed description of how William Henry Harrison's supporters in Pennsylvania and Maryland effectively destroyed Webster's chances in 1836. Of most interest to scholars will be the author's argument that Webster finally gave up hope for an alliance with Jackson not when Old Hickory decided to "remove the deposits" from the Bank but only when Van Buren prevailed on the president to repudiate the plan of cooperating with Webster and his supporters in the Twenty-third Congress.

Mr. Brown's book is not without flaws. The discussion of the Whig constituency is thin, noting only the economic characteristics stressed in studies of an earlier era. The statement that the poorer classes of Massachusetts "wanted a change in the established social order" is hardly illustrated by the behavior of those classes. More important, the book fails to fulfill the promise implicit in its title. The reader who hopes to find significant generalizations about presidential politics will be disappointed. Mr. Brown describes the words and actions of Webster and others, but he seldom explains or even tries to explain them. The

result is a book somewhat bloodless, not comparing in interest or dimension with Claude Fuess's *Webster* of a generation ago. Mr. Brown should have ventured more of his own judgments, for weaknesses or no, he has written a sensible and useful book.

EDWARD PESSEN
Baruch College,
City University of New York

GEORGE M. MARSDEN. *The Evangelical Mind and the New School Presbyterian Experience: A Case Study of Thought and Theology in Nineteenth-Century America*. (Yale Publications in American Studies, Number 20. Published under the direction of the American Studies Program.) New Haven: Yale University Press. 1970. Pp. xiii, 278. \$10.00.

Professor Marsden focuses on the New School Presbyterians to illustrate what he discerns to be a broader "transition from the theologically oriented and well-informed Calvinism characteristic of much of American Protestantism at the beginning of the nineteenth century to the nontheologically oriented and often poorly informed conservative Protestantism firmly established in middle-class America by the end of the same century" (p. 2). He traces the origins of the New School to a larger American evangelical awakening having national aspirations and closely identified with New England Congregationalism. The resulting variations of Calvinistic theology were primarily responsible for the Presbyterian schism in 1837. Soon the New School found the national evangelical emphasis on voluntary associations shrinking in the face of denominationalism. Needing a distinctive theology for prestige, the New School also faced intellectual challenges from developments in geology and biology as well as from encroachments of German philosophy. Primarily through the work of Henry B. Smith, New School Presbyterians met these challenges and in so doing moved closer to the orthodoxy of the old school. This development provided the possibility for Presbyterian reunion in 1869, although the nationalism engendered by the Civil War was a factor. By this time, however, there was little to distinguish the values of American Protestantism from those of the American middle class.

The volume supplies valuable insight into a leading denomination in a period for which there is no other recent historical account and touches on some of the most significant questions confronting American historians in recent decades. However, Professor Marsden's concern with theology and his denominational focus lead to a fundamental error. He falsely assumes that prior to the emergence of the New School "the American Churches clearly represented the Church militant, standing arrayed against a secular nation" (p. 241), but Reformed Protestantism had been closely identified with American culture for at least a century, most notably in the American Revolution. The significant question would involve the nature and function of this cultural identification both before and after 1837. The relationship of doctrines of salvation to emerging political capitalism is more central than the "erosion" of theology, and remains undelineated.

FRED J. HOOD
Georgetown College

JOSEPH FRAZIER WALL. *Andrew Carnegie*. New York: Oxford University Press. 1970. Pp. xii, 1137. \$15.00.

Andrew Carnegie is a remarkable achievement: a huge slice of American and British economic, political, and intellectual history served up in rich, leisurely detail. Immensely knowledgeable, the author ranges widely over the many lives of Andrew Carnegie, destroying myths, establishing connections, constructing a complex, sensitive portrait of the man and his times. Some diffuseness is inevitable in a thousand pages, noticeable particularly in the final chapters; yet the overall structure of the book is elegant indeed.

Carnegie's life-experience—1835–1919—was very nearly the American national experience in microcosm: classic humble origins in Dunfermline Chartism; emigration and uncritical adoption of the United States; an astonishing business career in which as a pioneer he made a personal impact on major industries at the very heart of America's rise to economic power—telegraphs, railroads, sleeping cars, oil, bridge-building, iron production, and finally steel; and then, leaving behind him the nation's

first billion dollar corporation, a "retirement" in order to spend twenty more years of public life laying the basis for a new industry of philanthropy through the mechanism of the foundation. Bare summary omits his work in the peace movement, his idiosyncratic anti-militarism and anti-imperialism (which took him to the quixotic point of almost buying the Philippines for \$20 million to liberate them from the United States), his deep involvement in American and British politics and journalism, his world travels and voluminous writings and speeches. Above all, Carnegie's private self-image was as a writer and thinker.

Dr. Wall opens new perspectives on major aspects of American history as he approaches each stage of Carnegie's career. His book stimulates so many second and third thoughts—about the emigré syndrome, the myth of social Darwinism, the myth of *laissez faire*, robber baronism, imperialism, all themes worthy of separate review essays. We see Carnegie playing the role of the radical dissident in Britain and of the vociferous conservative booster in the United States: wriggling uncomfortably on that special hook reserved for successful emigré radicals, at least until 1892 when Homestead destroyed his radical image in Britain. He suffered "arrested development" as a political thinker from the moment of his emigration—disciples of Louis Hartz's "fragment society" theory please note. We see this exemplar of social Darwinism get Herbert Spencer one hundred per cent wrong and disregard his ideas whenever useful. As for *laissez faire*, Carnegie played both sides against the middle, callously disregarding his own pooling arrangements to undercut a rival at one time, fighting zealously for state intervention to control the Pennsylvania Railroad at another.

There is so much more in this book. For instance, those economic historians who regard David Landes' *Bankers and Pashas* (1958) as the best students' introduction to the economics of international banking will be delighted with Wall's clear, nontechnical explanation of the economics and technology of nineteenth-century industry, from the dramatic Lucy blast furnace or the Bessemer process to the financial side of Carnegie's corporate empires. Fine sections of the book will please future anthologists: the Herbert Spencer visit, Homestead, the

struggle with Henry Frick. No doubt some readers will conclude more critically of Carnegie, given the book's own evidence: his attacks on speculation once his own speculative fortune was made; the complex pattern of collusion and interlocking by which he made himself rich in the 1860s and 1870s; his absentee leadership of Homestead; his cost-cutting fixation; his apparent emotional shallowness, seen in his dumping of Tom Scott in 1873 and in his discreditable attempt to cheat Frick later. Yet Dr. Wall gives all the evidence for such judgments and makes some himself along the way. One is still left with grudging admiration for Carnegie: with all his faults he seems more alive than Rockefeller, more human than Morgan.

Here is a book for the prize committees. To write Carnegie's life was a great challenge, successfully met by the author's learning, skill, and sensitivity. Comparable with works like Allan Nevins' Rockefeller and Mark Schorer's Sinclair Lewis, *Andrew Carnegie* will prove to be a classic American biography and a genuine contribution to American scholarship.

PETER D'A. JONES

*University of Illinois,
Chicago*

NORMAN L. CROCKETT. *The Woolen Industry of the Midwest*. [Lexington:] University Press of Kentucky. 1970. Pp. x, 149. \$7.25.

The Midwestern woolen industry experienced uncertain adolescent years from roughly 1840 to 1860. It came to maturity in the 1860s and reached its prime in 1870, when 881 mills constituted thirty-six per cent of all such factories in the United States. But by the 1880s its life was threatened by the national forces of an urbanism that depleted the rural markets, of style changes to worsted goods and ready-made clothes, of lower-cost and improved transportation that facilitated interregional competition, and of the ever westward migration of sheep. The great advantage of the Midwest woolen mill—its lower transportation costs to a local market—had gone with the growth of the nationalized market of the American economy. In 1900 only 183 mills remained; by 1920 they had dwindled to 70, only a few of which were not speciality mills. The birth, development, struc-

ture, and death of the industry has been competently described in this brief book; Professor Crockett searched eight Midwestern states for available manuscript sources and has used effectively, in an unexciting but clear prose, a considerable body of printed material.

Crockett writes that the woolen mills, as other local, small manufacturing, contributed little directly to the economic development of the Middle West as a region. But, the author says, the industry served a vital purpose within the transitional stage of the Midwestern economy in influencing the utilization and movement of capital and labor and in providing a useful and needed service. Ironically the industry then fell victim to the economic progress it had helped to create. In similar manner, Crockett's account is double edged. The greater value of the book lies not in its description of a small, dead industry, but in the way Crockett ties the history of the industry to the economic history of the Middle West and the United States. In this sense what by title looks like a highly specialized work becomes of value to many generalists.

JAMES A. HODGES
College of Wooster

ROYCE D. DELMATIER *et al.*, general editors. *The Rumble of California Politics, 1848-1970*. New York: John Wiley and Sons. 1970. Pp. ix, 483. Cloth \$9.95, paper \$5.95.

MICHAEL PAUL ROGIN and JOHN L. SHOVER. *Political Change in California: Critical Elections and Social Movements, 1890-1966*. (Contributions in American History, Number 5.) Westport, Conn.: Greenwood Publishing Corporation. 1970. Pp. xx, 231. \$12.50.

California's politics are as much a mystery to historians east of the Charles River as they are to natives of the state. While the Democrats have held an overwhelming advantage in party registration for nearly four decades, only three gubernatorial elections in this century were won by them. The explanation of Democratic impotence has never been plausible, though historians usually point to the practice of cross-filing that permitted popular and not so popular Republicans to beat the Democrats. Except for Ronald Reagan, who won his first election without previous experience, Republican candidates worked their ways through the

party organization by various patterns of activity. The Democrats, however, selected men lately arrived in California, men from outside the party, and men whose reputations were often splendid but whose personality or associations repelled the voter. While Republican supremacy has been challenged, the challenge came from within the party, in the form of Johnson Progressivism and Warren liberalism. In both cases the Republican leaders drew support from Democrats and had opposition from party regulars. Even faced with these threats to unity, Republicans have had the good sense or fortune to resist disintegration.

Both books are excellent studies of California politics. *The Rumble of California Politics* is a collection of thirteen well-edited essays, which are generally uniform in treatment but concentrate on developments since 1900. Approaches are traditional, informative, and descriptive. Most of the essays are well researched by historians and political scientists who have some written or published monographs in the field. Probably the best essays in terms of interest and research are those by Judson Grenier, "Hiram Johnson and the Progressive Years" and Royce Delmatier, "The Rebirth of the Democratic Party." Both authors think Progressivism was marvelously successful from 1911 to 1915 when most of the program was legislated, but then Johnson and his followers feuded and the governor sought escape into national politics. When he became United States Senator in 1917, he left the party without strong Progressive leadership. Even though his successors were more conservative than he was, they were not unseated by the Democrats who were so disunited by issues of race relations and liquor control that they could not put forth a liberal program.

These broad outlines of politics are contrasted by the restricted treatment of the six essays in *Political Change in California*. Rogin and Shover are exclusively concerned with election analysis of mass behavior. They investigate voting patterns in the tradition of V. O. Key, Jr. and point to various important elections in which the masses made deep political commitments. Their findings are most significant and challenge particularly the usual generalizations about political alignments during Johnson's Progressive years. These shifts in

votes may explain the tensions that Grenier described and that splintered Johnson's following.

Progressivism in California, according to Rogin and Shover, may have originated as a middle-class movement, but a shift in support occurred soon after Johnson took office. His support was drawn from the two metropolitan centers, and between 1911 and 1915 these voters rallied to his social program, while those living in the suburban areas of Pasadena and Pacific Heights withdrew their support. As a class full of immigrants, Roman Catholics, and city dwellers, they became sensitive of their political needs and developed methods for obtaining relief. The authors believe that there is a direct relation between these working people who backed the Progressives and the later supporters of the New Deal.

These workers were not drawn into the Democratic party because of the ineptness of its leadership. Besides their factional quarrels of the 1920s, Democrats in the 1930s waged vicious primary campaigns against each other and aided Republicans to win the general elections. The authors, however, are not satisfied that they have empirical evidence to explain why the Democrats always lost in the general elections. They think the press, which was overwhelmingly Republican, was an "important factor in conditioning political sympathies" because so many of the inhabitants were recent arrivals in California and party organization was too weak to maintain loyalty.

Behavioral studies such as this one have much to offer in the presentation of hard statistical evidence. While one misses the play of politics, the hatreds and rivalries, there is a concentration upon voting behavior that may explain political conduct. In their chapter on southern California the authors neglect to analyze the power of the *Los Angeles Times* or the influence of racial and national minorities upon politics. The omission of these topics may illustrate the limitations of this kind of research. Nonetheless, their observations on politics are worthy of serious consideration. They make a major contribution in analyzing election data and add a dimension of evidence to the understanding of California politics.

JOHN A. SCHUTZ

University of Southern California

DONALD E. REYNOLDS. *Editors Make War: Southern Newspapers in the Secession Crisis*. Nashville: Vanderbilt University Press. 1970. Pp. xi, 304. \$10.00.

In early 1860 Southern newspapers were predominantly Unionist, but one year later most of them were either advocating or justifying secession. Professor Reynold's aim is to demonstrate why and how this important shift occurred. Making use of nearly two hundred newspaper files, he investigates editorial reaction to the major political events of 1860-61—all four nominating conventions, the "Texas fires," Northern state elections in October, Lincoln's victory in November, the secession of the lower South, abortive attempts at compromise, the fall of Fort Sumter, and Lincoln's call for troops in April 1861.

According to Reynolds, several developments—especially an alleged abolitionist plot in Texas, the election of Lincoln, the secession of the cotton states, and the failure of compromise—"shifted the initiative to the disunion publicists and led to a gradual breakdown of Unionist journalism" during the autumn and winter of 1860. By April 1861 Southern newspapers, with only two or three exceptions, were a unit for secession. In addition to this major theme are several other conclusions. The "Texas fires" in the summer of 1860, supposedly set by abolitionists and rebellious slaves, were probably the result of spontaneous combustion. A majority or plurality of newspapers in ten Southern states supported Breckinridge; only in Tennessee was Bell as strong. Douglas placed a poor third throughout the region, and Lincoln was limited to two journals in western Virginia. Significantly, in every Confederate state but Virginia the candidate who won the newspaper race also won the electoral vote.

Reynolds implies that Southern editors, in exaggerating and distorting Northern views, were cynically shoving the public toward secession. Perhaps, but many journalists believed what they wrote, believed that Northern opinion was dangerous and aggressive. His treatments of the "Texas fires" and the relative press strengths of the presidential candidates are the most reliable yet published, but many of the author's other conclusions have appeared in earlier works, such as Ollinger Crenshaw's *The Slave States in the Presidential*

Election of 1860 (1945). Still, this is a useful book, particularly for its systematic analysis of evolving press opinion and its wide-ranging research. For lagniappe, a valuable appendix lists about two-thirds of the 850 Southern newspapers according to the candidate they supported in 1860.

RICHARD LOWE
North Texas State University

JAMES M. MERRILL. *Battle Flags South: The Story of the Civil War Navies on Western Waters*. Rutherford, N. J.: Fairleigh Dickinson University Press. 1970. Pp. 334. \$10.00.

It can be argued that one of the few advantages of having been a soldier in the Civil War was that it saved you from the outpouring of unedifying verbiage that so frequently passed for history during the celebration of the war's centennial. One of the curiosities of that outpouring was the scarcity of well-researched and competently written studies of naval operations. The extremely important activities of the Mississippi and Ohio rivers and their tributaries in particular received scant attention. James M. Merrill has both filled in a large part of that need and explained the dearth of earlier works in his *Battle Flags South*. To find materials beyond the official record he had to consult not only those snoring in dusty memoirs and monographs but collections of letters scattered from New Hampshire to California.

Professor Merrill writes with a deft pen and an eye on personalities. Indeed, this study of command is largely an account of the interaction of time, place, and events with personality. As a result its best chapters are those that deal with the frustrations encountered by both Union and Confederate naval leaders as they attempted to create squadrons with minimal support from superiors a thousand miles away. Yet by the end of 1861 the Union's makeshift gunboat flotilla had proven its value at the battle of Belmont and in the following spring came into its own at Forts Henry and Donelson. Strengthened by a series of ironclads built in large part by a man who had never before built a vessel, the Mississippi Squadron of the United States Navy joined with its deep-water and mud-caked compatriots to split the Confederacy in two and to ensure the eventual collapse

of the rebellion. This is the story that Professor Merrill tells in a book that will remain a standard account of the first two years of the war on the western waters. It is unfortunate, therefore, that the book lacks both pictures and maps.

K. JACK BAUER
Rensselaer Polytechnic Institute

WARREN W. HASSLER, JR. *Crisis at the Crossroads: The First Day at Gettysburg*. University: University of Alabama Press. 1970. Pp. viii, 214. \$8.75.

FRANK L. BYRNE and ANDREW T. WEAVER, editors. *Haskell of Gettysburg: His Life and Civil War Papers*. Madison: State Historical Society of Wisconsin. 1970. Pp. vii, 258. \$6.95.

The battle of Gettysburg, that dramatic old-style tournament of war with armies marshaling in open fields, banners flying and bayonets gleaming in the sun, has an antique quality that seems to infect even the most recent writing about it. We have become accustomed to the purple prose of Frank A. Haskell's eyewitness account; when he tells us that Union soldiers under Confederate artillery bombardment said, "O, this is bully," we feel skeptical, but we recognize the conventions of nineteenth-century literature and accept the narrative for the valuable document it is in spite of such conventions. It is a little more unsettling to find Warren W. Hassler in 1970 passing on to us, with his eyebrow unraised, reports that have soldiers rushing into battle with enthusiasm, shouting to their commander as they pass him that they will save McPherson's Grove, for, "If we cannot hold it, where will you find the men who can?" It is still more unsettling when Hassler himself indulges in prose more purple than Haskell's, writing about "the Valhalla of American military endeavor," in which "there is no brighter or more imperishable niche than that which is reserved for the unheralded soldiers of both sides who strove heroically and with extreme devotion for the cause they believed just in the crucial First Day's Battle of Gettysburg. Their conduct was indeed sublime." Perhaps so; but that kind of writing hardly enhances the cogency of military history.

Nevertheless, if Haskell's account has survived despite its strivings for literary effect, so will

Hassler's history, as our most detailed narrative of the first day's battle, examining the day's action so minutely that no succeeding historian of Gettysburg will be able to ignore it. Hassler's book has solid virtues in addition to its thoroughness of detail. It offers a persuasive argument that the first day's events largely determined the eventual outcome of the battle. Hassler displays uncommonly complete knowledge of the battlefield terrain. Along with Glenn Tucker, for example, he is one of the few historians to make clear the fact that McPherson Ridge is a double ridge with two crests, and this point alone clarifies much that is confusing in other narratives. Hassler makes uniquely good use of the information that can be gleaned from the monuments and markers on the battlefield. He may rely too much, however, on memoirs as sources, especially on those of Abner Doubleday, whom he makes a major hero of the first day's battle. If Doubleday performed as well as Hassler indicates after he succeeded to command of I Corps, it is strange that he allowed his own former division to be so badly deployed, or that he failed to notice that his successor in its command, Thomas Rowley, was drunk, as a court martial later found he was.

In the Haskell book we have the first scholarly edition of Haskell's account of Gettysburg printed from Haskell's manuscript. The text proves not to differ importantly from the Wisconsin History Commission edition of 1908 favored in other recent reprintings, and the differences are noted. There are also selections from Haskell's other Civil War letters (which are not nearly so useful or absorbing as his carefully polished Gettysburg letter), a competent sketch of his life, and an account of the publications of the Gettysburg letter.

RUSSELL F. WEIGLEY
Temple University

YUR-BOK LEE. *Diplomatic Relations between the United States and Korea, 1866-1887*. New York: Humanities Press. 1970. Pp. 211. \$7.50.

In this work Professor Lee discusses the early American attempts to open Korea, the major problems and personalities in Korean-American relations between 1882 and 1887, and the position of Korea vis-à-vis Japan and China

during the decline of the Manchu dynasty. Because Korea's traditional relationship with China complicated her association with the other powers, Lee, of necessity, places considerable emphasis on Sino-Korean developments.

While the author makes a contribution in his limited area, his book, in my judgment, suffers some important deficiencies: it is inappropriately titled, since Korean-American diplomatic relations did not really begin until after 1882; it is lacking in skillful organization and is awkwardly written, a problem compounded by the author's frequent omission of the definite article (which could have been corrected by an editor); and, more important, it could contain much more discussion on the role of Korean-American relations in the context of United States Far Eastern policy and the total American foreign policy. As Lee points out, for some eighteen months after January 1885 United States representation in Korea was left in the hands of a *chargé d'affaires* who in 1886 was replaced by a helpless alcoholic. For an extended period the American *chargé* did not receive funds to run the legation or, incidentally, for payment of his own salary. Such facts should elicit more comment about the State Department's concept of Korea's overall "importance," of its market potential, and of the role of the country in America's future position in the Far East.

Though the work possesses these shortcomings, it is nonetheless of some value as a new survey of Korean-American relations and a brief review of late nineteenth-century Korean history.

RUSSELL D. BUHITE
University of Oklahoma

CORNELIUS C. SMITH, JR. *Emilio Kosterlitzky: Eagle of Sonora and the Southwest Border*. (Frontier Military Series, Number 7.) Glendale, Calif.: Arthur H. Clark Company. 1970. Pp. 344. \$12.50.

In 1913 Colonel Emilio Kosterlitzky led his *Rurales* across the border into the United States after a battle with Alvaro Obregón in Nogales. Forty-one years earlier Kosterlitzky had deserted a Russian training ship and joined the Mexican army as a private. He served in the Apache and Yaqui-Mayo wars in the 1880s and rose rapidly in rank, and in 1885 he was

selected by Porfirio Diaz as an officer of the Gendarmeria Fiscal or *Rurales*. He also became chief of military intelligence for Sonora, Sinaloa, and part of Baja California and was one of Diaz's most loyal and enthusiastic subordinates. He was the "mailed fist" of Diaz and became famous for suppressing the strike in the American-owned copper mines at Cananea in 1906. Kosterlitzky continued to serve the government after the fall of Diaz until 1913. Then, after a period of internment he lived in Los Angeles and worked for the United States government, serving as an undercover agent during World War I.

If Emilio Kosterlitzky had lived on the American frontier, he would undoubtedly be well known, but he served in Sonora, an area that has not been thoroughly studied. In addition he was loyal to Diaz, whose tyrannical rule has been condemned by scholars. This biography provides a sketch of Kosterlitzky's career but fails to analyze it. Because of the limited documentary materials, Kosterlitzky is often a peripheral figure. He is rarely mentioned in the first third of the book while the last third is devoted to his years in the United States.

The author has relied heavily on the reminiscences of his father, who knew Kosterlitzky; he has also utilized the papers owned by the Kosterlitzky family. If additional material exists in the military records of Mexico, it has not been exploited. The author fails to answer many of the questions raised in this rather loose and informal narrative. He does not evaluate the role of the *Rurales* or the influence that Kosterlitzky may have had on their development. He also avoids passing moral judgments and sidesteps the question of whether loyalty and obedience justify summary executions of suspected criminals or opponents of the establishment.

RICHARD ELLIS

University of New Mexico

DAVID HEALY. *US Expansionism: The Imperialist Urge in the 1890s*. Madison: University of Wisconsin Press. 1970. Pp. ix, 315. \$10.95.

E. BERKELEY TOMPKINS. *Anti-Imperialism in the United States: The Great Debate, 1890-1920*. Philadelphia: University of Pennsylvania Press. 1970. Pp. 344. \$12.50.

Under the probable inspiration of contemporary events in Southeast Asia, the last decade has seen a revival of scholarly interest in American imperialist activity of the 1890s and in the debate it inspired. The labors of May, Freidel, Beisner, McCormick, and others are now augmented by the complementary studies of E. Berkeley Tompkins and David Healy.

Each of the books under discussion is a well-executed piece of historical analysis, but each gains an additional dimension when read in the context of the other. Professor Healy's primary concern is the intellectual and economic origins of "the imperialist urge in the 1890's"; Professor Tompkins' major emphasis is upon the ideas and organizational efforts of the anti-imperialist movement. Supplementary in their respective emphases, these volumes offer collectively the most satisfactory interpretation available of the origins and consequences of the Great Debate over the acquisition of an American colonial empire.

Not only are these volumes complementary in their respective emphases, but they share various attributes. Each of these scholars has examined a wide range of source materials, and each has had the good sense to disguise the extent of his labor in a prose style of clarity and grace. Healy and Tompkins agree, furthermore, that the debate between the expansionists and the anti-imperialists represented a division of substance not an exercise in shadowboxing and that the consequences of imperialist victory were of major significance for the course of American historical development. Though with different shadings, each would appear to agree that America's decision to embark on a course of insular expansion in the Pacific and to establish thereby a power commitment outside the Western Hemisphere was—for all its foreshadowing in the history of our commercial relations with the Far East—"a new departure."

These volumes offer elements of contrast as well as complementarism. Healy would place the Great Debate in sharper economic focus than does Tompkins; Tompkins is more certain of the distinction to be made between the racist beliefs of imperialists and anti-imperialists than is Healy. Their chief contrast, however, lies in their respective design.

Healy attempts a difficult format and succeeds

admirably. He juxtaposes theme chapters and biographical sketches of five men who exemplify those themes—James Harrison Wilson, Theodore Roosevelt, Elihu Root, Charles Denby, and Charles Conant. Largely as a result of his literary skill, Healy is able to avoid the pitfall of disjointedness and to persuade the reader of the interrelation of the goals and motives underlying the imperialist argument. Economic ambitions are given the most extended treatment, but Healy does not fall prey to the new economic determinism. The influence of European thought and practice as well as the ideological and psychological assumptions of expansionism are given detailed illustration in what is an excellent synthesis of the convictions and self-delusions of imperialist thought. One might argue that Healy's estimate of the sophistication of Charles Conant's economic philosophy is rather high, but it would be difficult to fault Healy's selection of biographical examples or the balance of his appraisal of elements of "conflict and consensus" among opponents in the Great Debate.

The design of Tompkins' study is more traditional. He examines in straightforward, chronological fashion the sequence of diplomatic events and issues that inspired the anti-imperialist movement and its successive battles, strategies, and failures. The major contribution of his study lies in its careful and comprehensive examination of the organizational history of the movement, the character of its membership, and the multifaceted quality of the anti-imperialist argument. Tompkins makes clear that moralism and nostalgia were but part of the anti-imperialist rationale and that the anti-imperialist leadership did not ignore the economic and strategic dangers of colonialism. If one may quibble over Tompkins' optimistic judgment respecting the ultimate success of the anti-imperialist movement, students of the Anti-Imperialist League can only express their gratitude that they need no longer rely on the rather confused analysis and uncertain statistics of Maria Carpio Lanzar.

Students of American expansionism may indeed see the books of Tompkins and Healy as fair witness that a reinterpretation of our imperial past is now well under way. These two analyses of the interplay of ideas and

diplomatic practice contribute significantly to that reinterpretation.

RICHARD E. WELCH, JR.
Lafayette College

HAROLD S. WILSON. *McClure's Magazine and the Muckrackers*. Princeton: Princeton University Press. 1970. Pp. ix, 347. \$10.00.

Harold Wilson has written two books, not one. The first is a mini-biography of S. S. McClure—or, more accurately, of the magazine that bore his name. The second is an analysis of the intellectual and philosophical underpinnings of the thinking and writing of the men and women whose muckraking activities were identified with *McClure's*. The relationship between Sam McClure's magazine and those who wrote for it deserves the careful consideration the author has given it. But he might have been well advised to develop each aspect separately in the interest of doing justice to both. This book inevitably invites comparison with others that have approached the man, the magazine, and the muckrakers separately. Peter Lyon's *Success Story* (1963) and Louis Filler's *Crusaders for American Liberalism* (1939) come immediately to mind, and though Wilson offers new information and fresh interpretations, his book lacks both the sweep and the depth of Lyon and Filler.

A conscientious scholar, the author never strays far from his sources, using primary materials extensively and often effectively. But at times he ties himself so closely to these documents that one wonders why he did not simply offer them with annotations. This would have avoided the festoons of footnotes that divert one's attention from the text to a veritable garland of *ibids*.

What Wilson offers that is useful to both historians and journalists is further examination of what was going on in the minds of Lincoln Steffens, Ray Stannard Baker, Ida Tarbell, and others of the *McClure's* group. Their thought was not superficial, as has sometimes been alleged, and his latter chapters are both challenging and stimulating in their analysis of what the muckrakers thought they were trying to accomplish.

The writing is turgid, and the author is given to coy devices of the sort one hoped had

gone out with the bustle. For example, William Allen White is rarely mentioned by name until there has rarely been a reference to him as the "voice from Emporia," or some similar indirect allusion.

There is one final bonus—the brief but informative set of bibliographical notes. Though it does not include everything that might have been catalogued it is perhaps the best critical assessment to date.

JOHN M. HARRISON
Pennsylvania State University

WALTER I. TRATTNER. *Crusade for the Children: A History of the National Child Labor Committee and Child Labor Reform in America*. Chicago: Quadrangle Books. 1970. Pp. 319. \$10.00.

This study outlines the history of child labor in the modern American economy and describes the efforts of the National Child Labor Committee to abolish it. From the beginning of settlement in America child labor was one of several solutions to the chronic scarcity of labor in the New World; in the nineteenth century rapid economic development greatly increased the demand for labor and by the first decade of the twentieth century more than two million children under the age of fifteen worked long hours for low wages under hazardous and debilitating conditions in industry, agriculture, and in the so-called street trades. Since then the trend has been gradually reversed; the increasing mechanization of routine tasks, the rapid expansion of the economy's service sector with its need for educated and mature workers, and the general rise in the real wages of working-class families have combined with other factors to place ever more restricted limits on the market for young workers. Child labor is now confined largely to migratory farm work and to street trades like news vending. In short, deep-seated social and economic forces first created child labor and then provided for its inevitable abolition.

Nevertheless the inevitable had to have help to achieve its ineluctable ends. In 1904 a reform coalition representing every class and section of the country formed the National Child Labor Committee. Staffed by professional social workers, the committee produced detailed

studies of child labor in America, sought to generate public sentiment in favor of reform, and lobbied first for state and then for national legislation against the evil. Child labor, the reformers argued, was degrading and uneconomical. It denied American children the right to develop themselves to the outer limits of their potential. Children were inefficient workers whose employment wasted the nation's resources. Excessive toil under hazardous conditions destroyed their health and weakened their morals; by denying them formal education it threatened to turn them into a permanent class of paupers. Diseased, degenerate, and destitute children endangered the future stability of American society and, in an argument especially but not exclusively popular in the South, might eventually contribute to the suicide of the white race. At bottom child labor was seen as a problem of social control and community cohesion. By mid-century the committee had won its legislative and propaganda battles; legal obstacles blocked most forms of child labor and no less a pillar of American respectability than President Eisenhower sent the committee an encomium on its fiftieth birthday.

Trattner's intelligent and well-researched history is a valuable contribution that might have been even better had he interpreted the rise of child labor in light of the emergence of the child in modern societies as a separate social category and a separate social problem. The book could also have been improved by a discussion of its bearing on the general historiography of the Progressive era. Although he has uncovered evidence to support the divergent and often conflicting hypotheses of George Mowry, Samuel P. Hays, Gabriel Kolko, Robert Wiebe, J. Joseph Huthmacher, and Christopher Lasch, Trattner neglected the opportunity to comment on the adequacy of these rival interpretations. If anything, however, his study supports the recent contention of Peter Filene ("An Obituary for 'The Progressive Movement,'" *American Quarterly*, 22 [1970]: 20-34) that Progressive reforms were characteristically effected by shifting coalitions around different issues.

THOMAS A. KRUEGER
*University of Illinois,
Urbana-Champaign*

JAMES R. KLUGER. *The Clifton-Morenci Strike: Labor Difficulty in Arizona, 1915-1916*. Tucson: University of Arizona Press. 1970. Pp. 94. \$3.50.

The three companies that owned the Clifton-Morenci copper-mining district in Arizona during the early 1900s netted fantastic profits. In 1912, for instance, the Detroit Mining Company paid a dividend of 146 per cent on its capitalization of a million dollars. At the same time this company and the two others paid their employees, seventy per cent of whom were Mexicans or Yaqui Indians, the lowest wages in Arizona. Out of their meager salaries these men had to bribe minor officials in order to keep their jobs. Eighty per cent of the workers were in debt to their employers' mercantile departments. Years of abuse prompted the men to come out of the shafts in September 1915 in a hastily called strike. After a few meetings the managers shut down all operations and moved to El Paso.

Although the miners beat up several men, only one was killed, making this the most non-violent of all the strikes in the mining states during this time of labor upheaval. When the impasse finally ended the following January the miners enjoyed a new wage scale but agreed not to join the Western Federation of Miners. They could and did, however, join the Arizona State Federation of Labor.

This is an interesting, well-told story. But only Governor G. W. P. Hunt, who attempted to mediate, emerges as a person. The account also leaves a number of questions unanswered. For example, what other factors caused the laboring man's governor to win re-election only after a recount? Sheriff James G. Cash appears as one of those most responsible for keeping the strike peaceful, yet it is not clear why he lost his bid for re-election. Despite these omissions Kluger has written a scholarly little volume that will be of great value in piecing together a more complete story of twentieth-century Arizona, the labor movement, and the American West.

JIM B. PEARSON
*University of Texas,
Austin*

CHARLES GILBERT. *American Financing of World War I*. (Contributions in Economics and Economic History, Number 1.) Westport, Conn.: Greenwood Publishing Corporation. 1970. Pp. xix, 259. \$9.50.

Although the historical literature concerning America's participation in the First World War is large, it is sparse in regard to Wilson's domestic mobilization programs during these years. Indeed, many phases of the process whereby Americans mobilized are still somewhat obscure. This new book is welcome, therefore, since it is the first comprehensive work to deal with federal financial policies in the war era. Its author is not a professional historian but an associate professor of finance at Hofstra University.

Professor Gilbert has written a detailed and interesting account. His study contains an excellent, succinct survey of major problems of war finance that the Wilson administration had to surmount. These included the making of difficult choices between taxation and borrowing as well as the selection of alternative methods of taxation. The nation's leaders also sought to maintain some balance between long- and short-term debts and faced the hard task of compromising the demands of politics with economic needs. Much of the discussion in this monograph is devoted to an examination of the development of Wilsonian policies concerning these issues. Gilbert concludes that the Treasury Department could have been much more effective in many spheres but that it did as well as might be expected under prevailing conditions of the times. Yet he hopes that future generations will benefit from a clearer understanding of the shortcomings of World War I finance.

Despite many virtues this is not a definitive book on the subject. Professor Gilbert has based his study almost entirely on printed sources and has ignored a vast amount of relevant unpublished materials. Examination of the manuscript collections of William McAdoo and other important individuals who shaped the federal financial program, for example, or Treasury records in the National Archives would have added much depth to the story. Since the role of individuals and their interaction in the formulation of financial policies receives little attention, Gilbert bypasses an important dimension of American financial policies. By focusing primarily on what happened and much less on how and why policies developed he omits a note of realism.

One substantive error must be noted. Professor Gilbert chastizes Congress for not using

its tax powers in wartime to spur production of necessary materials (p. 233). In this he is mistaken. Congress made special provisions in the War Revenue Act of 1918 to encourage oil and minerals producers by allowing them to deduct a high percentage of their exploration and drilling expenses over a short period of time. The clause was intended primarily to encourage producers to boost their output.

But criticisms should not obscure the conclusion that this is an altogether useful volume, one of the first serious works in the field. Historians will welcome the sound and sensible judgments of the author. If his is not the final word on the subject, he has nevertheless opened many vistas that should lead others to give it the attention it has long deserved. Professor Gilbert has made a worthwhile contribution to an underdeveloped area of American financial history.

GERALD D. NASH
University of New Mexico

CLAUDIA KROMER. *Die Vereinigten Staaten von Amerika und die Frage Kärnten, 1918–1920.* (Aus Forschung und Kunst, Number 7.) Flaggenfurt: Geschichtsverein für Kärnten; distrib. by Rudolf Habelt Verlag GmbH, Bonn. 1970. Pp. 268. Cloth DM 35, paper DM 31.

Boundary problems first challenged, then bedeviled, and eventually haunted the experts who sailed with Wilson to Paris to draw the map of Europe according to the Fourteen Points. Those few who had some awareness of the problems of Austria-Hungary, until then an area outside the American field of interest, undertook to sort out the "ethnic salad" of the old empire. Drawing the boundaries of such places as Teschen, Carinthia, Silesia, and Slovakia followed the same pattern. Experts were sent on fact-finding tours to the many trouble spots, but their recommendations often went unheeded. When armed clashes followed, the territorial disputes were referred to special committees, whose deliberations only mirrored the differences among the Big Four. American experts were often divided on the question of self-determination; when pursued in the remote corners of Austria-Hungary this doctrine resembled the treacherous light that, according to local tales, leads unwary strangers into dangerous bogs. Wilson and House, haunted by difficulties at home and by fear of revolution

in Europe, often overruled the experts and traded territory for Allied unity and progress on the peace treaties. Stormy objections from friends and foes sometimes induced last minute compromises that contradicted all expert advice. The plebiscite was the last resort of the Big Four to escape from their dilemma.

The decision to hold a plebiscite in Carinthia, the wedge of land between Yugoslavia and Austria, was reached in this manner. Claudia Kromer, in her study of *The United States and the Question of Carinthia*, traces through American documents the discussions, reports, meetings, intrigues, and plots that resulted in the incorporation of Carinthia into Austria. Her treatment of the subject is less than objective and somewhat uncritical, omitting any discussion of the forces that shaped the outcome of the plebiscite and ultimately gave this territory to Austria. Though she espouses the Austrian point of view, she fails to examine the substance of either the Austrian or Yugoslav claims. Lieutenant Colonel Sherman Miles, son of the chief of staff of the U.S. Army and later head of G2 during Pearl Harbor, is the hero of the book. In 1919 he briefly visited snow-bound Carinthian villages to question inhabitants about their preferred allegiance. His report that much of the territory should be Austrian and that those who opted for Austria made "a better impression as men," is accepted by the author as proof of American "objectivity." Robert Kerner, a Czech ethnic from the Midwest who traveled the same route and disputed Miles' opinions, is dismissed as a victim of "Yugoslav propaganda." Well-researched and competently written, the book is an additional, if limited and somewhat parochial, footnote to the history of the Paris Peace Conference.

DAGMAR HORNA PERMAN
Chevy Chase, Maryland

MICHAEL E. PARRISH. *Securities Regulation and the New Deal.* (Yale Historical Publications. Miscellany Number 93.) New Haven: Yale University Press. 1970. Pp. x, 270. \$8.75.

In the author's view, New Deal programs for regulating the financial district constituted "a conservative revolution which nonetheless horrified a great many conservatives." "Franklin Roosevelt's administration, the Congress, and a diverse business community, all fervent

spokesmen for capitalism as a rampart of democracy, engaged in a seven-year struggle over how best to preserve capitalism." This much we are told in the introduction. Mr. Parrish goes on to document his case, and he does so convincingly. He has examined all the major primary sources on the government side of the fence and presents his findings in interesting and convincing fashion. Indeed, this short book is the best study of the subject yet published.

The thesis is by no means new. Consensus historians of the 1950s and revisionists of the late 1960s have both taken pains to report that the New Deal was essentially conservative, geared to preserving capitalism while reforming it. Roosevelt was convinced that the financial district was in need of drastic overhauling, but he was ever willing to compromise. Congressional reformers of an antibusiness bent, such as Senator Hiram Johnson of California, wished to enact a sweeping measure that would all but make the district a quasi-public utility. But New Deal moderates, farsighted businessmen, and above all, the flexible president had their way. The result was a set of bills designed to correct specific abuses, "put a policeman at the corner of Broad and Wall," and insist on complete disclosure of certain kinds of information. None of these goals were fully realized; the policeman was never adequately armed. Mr. Parrish has written an evenhanded book, but it is clear from its tone he would have preferred the Johnson approach.

The book's most valuable section is the chapters on "The Strange Death of Title II" and "The Origins of the Securities and Exchange Commission." A close reading of these would be of great value to those in government and finance who today are attempting to rewrite the laws of the 1930s. Had Title II been enacted, for example, some important problems that surfaced in the 1960s regarding conglomerate mergers and the responsibility of member firms to the public might have been avoided. Similarly, the author's account of the origins of the SEC and the way it conducted business shows the kind of interplay of men and ideas that usually takes place when government and the financial district combine to enact regulatory codes.

The work is not without weaknesses. Mr.

Parrish's account of the pre-1933 attempts at reform is weak. He ignores the efforts of the New York attorney-general, Albert Ottinger, to expose bucket shops in the 1920s and in ending the life of the infamous Consolidated Stock Exchange. Ottinger's work was considered important enough to win him the GOP gubernatorial nomination in 1928, but he lost to Roosevelt in a close race. The author's account of the Martin Act, New York's antibucket shop law, is incomplete, and elsewhere he shows little understanding of power relations in the district in pre-New Deal days. His analysis of the work of SEC leaders such as James M. Landis and William O. Douglas is likewise poor; a case could be made that neither had the kind of knowledge needed for the job. On the other hand, the book contains few errors of fact; Mr. Parrish has done his work well. He spells Samuel Untermyer's name "Untermeyer" and overestimates his impact on the financial community, but such errors are rare and minor.

Securities Regulation and the New Deal was written from the government's point of view, but the financial district also deserves a hearing. Vincent Carosso's *Investment Banking in America: A History* (1970) contains such an account, but it appeared after Parrish had completed his work. It might be read in conjunction with the present volume, however, and for that purpose.

ROBERT SOBEL

New College of Hofstra

C. K. MCFARLAND. *Roosevelt, Lewis, and the New Deal, 1933-1940*. (Texas Christian University Monographs in History and Culture, Number 7.) Fort Worth: Texas Christian University. 1970. Pp. viii, 132. \$3.50.

In studying the relationship between two of the key personalities of the 1930s, Professor McFarland had an excellent idea, one that, properly executed, could have provided new insights about leadership, power, and the New Deal's vacillating labor policy. It is regrettable that he has not executed the idea well. The work under review is only a superficial treatment put together mostly from published sources, marred by factual errors and faulty organization, and limited chiefly to narrative description that says little beyond what is found in standard surveys. Part of this failure

seems attributable to carelessness and inadequate research. Errors of detail keep appearing. (See, for example, the statements on pages 27–30 concerning the NRRB, the NIRB, and the EPIC movement.) Major labor collections have been left unconsulted. And post-1963 scholarship has been almost totally ignored. Perhaps the greatest difficulty, however, is conceptual in nature. The author, by and large, seems unaware of the questions he should be asking, of recent debates about his subject, or of the broader context into which his study should fit.

In its present form this monograph has little value. It serves primarily as an example of the type of work accepted for publication when proliferating university presses begin lowering standards.

ELLIS W. HAWLEY
University of Iowa

EDWIN P. HARTMAN. *Adventures in Research: A History of Ames Research Center, 1940–1965*. (NASA SP-4302. NASA Center History Series.) Washington: Scientific and Technical Information Division, Office of Technology Utilization, National Aeronautics and Space Administration. 1970. Pp. xviii, 555. \$4.75.

This official history recounts the activities of the Ames Research Center at Moffet Field, California, during its first twenty-five years. Its author is a retired engineer whose career was spent in a research and administrative capacity with the center's parent organization, The National Advisory Committee for Aeronautics (NACA). By his own admission he is no historian, but "an engineer-author who appeared bent on flouting every rule of conventional history writing." His self-indictment is too severe. Engineers, like historians, must be faithful to their evidence. For the most part he has been faithful. In consequence, the reader will find here an objective account of the growth of the center, of the evolving organizational structure, of the development of research facilities (wind tunnels, gas guns, and arc tunnels, for example), and of the range and changing nature of the research done. The last of these, of course, determined the changing character of the operation as a whole. Founded in 1940 on the eve of American entry into World War II, the center had to devote itself, during its

first five years, almost exclusively to wartime exigencies. Even so, its contribution to victory was not great. It had to build facilities before it could engage in significant research; and, besides, the planes that were to fight the war had already been developed. After the war the character of research activity shifted from the developmental toward the fundamental, a change that both the author and the center seem to approve. The postwar period emerges as a sort of golden age, despite considerable frustration because Congress refused to appropriate research funds sufficient to meet the Soviet challenge to American air supremacy. The launching of sputnik in 1957, however, brought new funds and an accelerated involvement in space research. The next year, NACA was taken up into the National Aeronautics and Space Administration (NASA), and in 1961 President Kennedy declared the national intention of landing a man on the moon by 1970. The advantages of increased funds were offset in part by loss of *esprit de corps* at the center because of growing complexity of organization and disparity of research projects undertaken. The original interdisciplinary community was reorganized along disciplinary lines. The major deficiency of the book as a historical work derives less from the author's engineering background than from his administrative point of view. Despite an intention announced in the preface to preserve the "motivating influences, and human experiences that tie the cold facts together," the author betrays in full measure an administrator's reluctance to speak publicly of internal stresses and strains. The reader seems to be offered here what management might deem acceptable for publication. Little light is shed on the real inner workings of a scientific bureaucracy.

LLOYD R. SORENSON
University of Oregon

JOHN HUTCHINSON. *The Imperfect Union: A History of Corruption in American Trade Unions*. New York: E. P. Dutton and Company. 1970. Pp. 477. \$12.50.

By the 1950s and 1960s many Americans had begun to believe that James Hoffa and his associates were the new robber barons because of the devastating findings concerning corruption discovered by the McClellan Committee.

This committee helped send Hoffa to jail; it also launched the career of Robert Kennedy.

John Hutchinson, a professor of industrial relations at the University of California in Los Angeles and an astute student of American labor history and industrial relations, has written a thorough analysis of corruption in the American labor movement. He starts his discussion by examining the corruption in the building trades unions, needle trades, the waterfront, culinary trades, building services, and theatrical employees. He then looks at what Congress has done to explore the nature of corruption in American labor and into the legislation that followed in the wake of these investigations.

But Hutchinson is not only concerned with what the public did to correct or to identify the abuses; he is also interested in what the labor movement itself did to handle trade-union corruption. He notes that in the beginning the labor movement did very little. But when the AFL-CIO merged, George Meany, the dynamic and strong-willed head of the new federation, decided to ferret out crime in the labor movement and took action. Meany's commitment, as well as the legislation passed by Congress, has thereby helped to reduce the amount of corruption found in unions.

Hutchinson also analyzes the reasons behind corruption in American labor. He notes that business unionism qua business unionism is not the origin of corruption; he recognizes that democracy will not mean that unions will be honest. Social unionism and democracy in labor are important in and of themselves, not necessarily to prevent corruption. Hutchinson's study indicates the kinds of unions that were more likely to be corrupt than others. They tended to be in industries that have "small business units, high proportional labor costs, small profit margins, intensive competition, and a considerable rate of business failures." In such industries it was easy for unions and companies to make deals. Certain industries also had special characteristics that permitted corruption to prevail.

Thus in his well-written study of corruption in American labor Professor Hutchinson has tried to analyze from a historical vantage point those factors that seem to differentiate the honest from the dishonest union. He has made a

start toward developing some generalizations that may eventually be the basis for policy.

ALBERT A. BLUM

Michigan State University

J. ERIC S. THOMPSON. *Maya History and Religion*. (The Civilization of the American Indian Series, Volume 99.) Norman: University of Oklahoma Press. 1970. Pp. xxx, 415. \$7.50.

Nine essays on lowland Maya secular and religious history constitute this ninety-ninth volume in The Civilization of the American Indian series. Thompson's multidisciplinary approach integrates evidence derived from archeology, colonial documents, and twentieth-century ethnographic investigations, and produces a balanced interpretation of the evidence.

Three chapters treat pre-Conquest subjects: Putun (formerly Chontal) Maya imperialism, the eastern Maya boundary, and Maya highlands-lowlands trade relations. Thompson's ingenious interpretation of the Putun Maya's role revitalizes the phrase "New Empire." in lowland Maya history. This dynamic group, facing both Nahuatl- and Maya-speaking peoples, were strategically located in the Grijalva-Usumacinta delta and Acalan province. These New World "Phoenicians" dominated sea-trading routes encircling Yucatan and reaching southward to Honduras' Sula Plain. Commercial pre-eminence led to political imperialism: between A.D. 850 and 950 the Putun Mayas seized control of northern Tabasco, southern Campeche, Cozumel, Chichen Itza, and territory drained by the Paisón and Belize rivers. In parts of this territory their rule continued until 1500. The Putun introduced eroticism into Maya religion. Their economic impact can be seen in the trade relations described by Thompson, the routes traced, and the precise list of trade goods given. The Putun area of operation is further delimited by Thompson's admittedly speculative definition of the Maya central area's eastern boundary, an excellent example of his historical method. Linguistic and archeological evidence combine to trace a Chorti Maya push westward into the highlands of Guatemala, Honduras, and El Salvador, staking out the line.

For the Conquest period, Yucatecan demography is studied. The widespread, yet sparse, population drastically decreased after the Con-

quest due to endemic and newly introduced diseases, economic collapse, flight from Spanish control, and annihilation.

Four chapters systematize the Mayas' complicated religion and surpass Thompson's earlier treatment of this subject in *The Rise and Fall of Maya Civilization* (1954, 1966). He clarifies the contractual nature of Maya religion, its hierarchy, its reliance upon sacrifice, magic, and divination, its insistence upon ritual purity, and its use of images, prayers, and hallucinatory drugs. Over 130 pages are required to explain the major and minor gods of the multitudinous pantheon. The quaternitarian aspects of Maya theology are well illustrated in the discussion of the Itzam Na cult. Within the 250 names and titles of lowland gods catalogued are gods of occupations, ancestral deities, and even imported foreign gods. Finally, Maya creation myths, with their numerous creative and destructive acts, are surveyed. Chapter 4, on the religious and medical uses of tobacco, might well have served as a transition from the secular to religious considerations of the book.

Doubtless Thompson's work, method, and historiographical example, enlightening in winning fashion, will stimulate further research. His apt analogies, whether drawn from primitive Yahwism or other sources, give memorable insight. His economical choice of illustrations—17 plates, 10 figures, 3 maps—fully exploited in the text, shows commendable awareness of the reader's needs. Under his hand, the Maya past lives again.

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SEYMOUR B. LIEBMAN. *The Jews in New Spain: Faith, Flame, and the Inquisition*. Coral Gables, Fla.: University of Miami Press. 1970. Pp. 381. \$12.50.

Several hundred Jews or crypto-Jews were living in Mexico City and its environs in the middle years of the sixteenth century, and the number seems to have increased steadily during the next few generations. Three sects, corresponding roughly to the modern Orthodox, Conservative, and Reform, are identifiable in the seventeenth century, before attrition and absorption brought a virtual end to colonial

Mexican Judaism some time prior to independence. The historian confronts numerous problems in the reconstruction of the life of this secret minority, whose inner religious integrity was strong but whose social position could never be manifested in any public way. Most of our knowledge comes from the records of the persons who were caught by the Inquisition, and as would be expected the material is richer on the details of individual cases than on the analysis of the group as a whole. It should be noted that modern Mexican Jews have a different history and are not related to those discussed here.

Professor Liebman has written previously on aspects of Mexican Judaism, particularly on the life and writings of the best-known figure, the "god-intoxicated" Luis de Carvajal. The present book contains an informative chapter on the Carvajal family and its relations in the late sixteenth century. Tomás Treviño de Sobremonte, a traveling merchant and manufacturer of cochineal, emerges as the outstanding figure of the seventeenth century. We see him as a proud Spaniard, ridiculing his Portuguese accusers (for a time the term Portuguese was practically synonymous with the term Jew), moving like some picaresque adventurer from one critical incident to another, and finally going to the stake defiant in his belief. Other portions of the work discuss particular Jewish practices, a secret code, the observance of holy days, and the covert desecration of Christian symbols. Appendixes catalog the persons who were reconciled and punished in the auto-da-fé of 1596 and in cases of the seventeenth century, especially of the period 1646–48. Though somewhat diffuse, the work is scholarly, with citations, glossary, and bibliography. Its tone combines objectivity with a sense of tragic dedication. A notable defect is that every page of the index contains errors in alphabetization.

CHARLES GIBSON
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JOSEFINA VÁZQUEZ DE KNAUTH. *Nacionalismo y educación en México*. (Centro de Estudios Históricos, New Series, Number 9.) [México, D. F.:] Colegio de México. 1970. Pp. vii, 291.

This remarkable book is both a general treat-

ment of a broad and significant topic and a more confined monograph. Within five separate, but not always conventional, time periods from 1821 to the present, the author provides an excellent survey of Mexican educational thought and policy, followed by a closer focus on the teaching of history as revealed in textbooks. The overall theme of the study is nationalism, and the more specific treatment of *historia patria* gives new insight into this protean phenomenon. Though she sees social coherence and a surmounting of traditional ideological disputes as a major aim and positive accomplishment of recent educational policy, she treats the matter critically and dispassionately, avoiding the all too frequent progressivistic clichés.

Her study provides impressive new evidence of the analogies between the late Porfiriato and post-1940 Mexico. She emphasizes the work of that "incredible group of pedagogues" who in the years following the National Congresses on Education (1889-91) sought a uniform, centrally controlled educational system. This system was to disseminate a single version of Mexican history, emphasizing evolution toward social fusion and peace. The author regards the creation of the free and uniform textbook program in 1959 as a realization of this earlier dream. The author is probably at her best, however, in dealing with the revolutionary era. Though the break with the old regime in education generally came in 1920, the conciliatory views of Justo Sierra prevailed in the history textbooks until 1926. Then, the old Hispanicist-Indianist conflict in interpretation re-emerged and was expanded by the "socialist" revisions of the 1930s. It declined after 1940 but saw its final florescence when the bones of Cortes and then of Cuauhtemoc were discovered in the late forties. Throughout this era the author has come upon topics worthy of further study.

Fresh glimpses of familiar figures appear at many points in the book. Despite somewhat fragmentary coverage of the pre-Reforma era, Carlos María de Bustamante emerges as a national mythmaker of major importance. The continuing impact of Sierra becomes more understandable. We also get a new view of José Vasconcelos, Manuel Gamio, and Samuel Ramos. In short, this is a mature and engaging

study, and it should be made available in English.

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CAMILO RIAÑO. *La campaña libertadora de 1819*. Preface by ALBERTO LEE LÓPEZ, O.F.M. (Sesquicentenario de la Campaña Libertadora de 1819.) Bogotá: [Ministerio de Obras Públicas] Comisión Especial Asesora. 1969. Pp. 312.

The best feature of this sesquicentennial commemorative book is its extensive quotation from both printed and unprinted documents from Colombia and Spain. Considering that the author is an army officer, one would expect outstanding maps. Regrettably this is not so. Much better maps on the campaign of 1819 are found in Colonel Alberto Lozano Cleves's *Así se hizo la Independencia*, volume 2 (1961). The black and white photographs of the bridge of Boyacá and of nearby battle areas are also disappointingly unclear. The nine portraits of the royalist and patriot officers involved are much clearer than the maps.

This book is very heavy with long quotations from Daniel F. O'Leary, from the Archivo General de Indias (Seville), especially from a set of documents copied by Salamanca Aguilera, and from articles and documents copied by Osvaldo Díaz Díaz, but it is rather short on interpretation. Many disputed issues involving critical decisions of Bolívar and Santander are described in detail, with extensive listing of numbers of troops, cannon, and cavalry involved, but with much too brief comment on the strategy and on the long-range significance of the decisions made by the commanders.

As tactical military history—well documented, using frequent references to good sources and secondary materials—this is a valuable book on the critical campaign that won freedom for New Granada in 1819 and marked the beginning of the end of Spain's great empire in the Americas. As might be expected, Bolívar emerges as the traditional hero. Santander plays his major supporting role, and his shooting of thirty-nine Spanish prisoners is defended—but not with enthusiasm. Pablo Morillo, the Spanish general, and Miguel de La Torre, his successor, are both treated better than in earlier chauvinistic Colombian writings. José Antonio Páez and the Venezuelan

generals are perhaps not given as much credit as they deserve for defeating the Spanish in 1819.

This is a good book for military history buffs. The general reader, interested in personalities, in the comedy of human errors, and in diplomatic, economic, and social history of this era will go elsewhere for his reading pleasure, especially to the biographers: to Salvador de Madariaga, Gerhardt Masur, and Daniel Joseph Clinton ("Thomas Rourke").

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GILBERTO FREYRE. *Order and Progress: Brazil from Monarchy to Republic*. Edited and translated from the Portuguese by ROB W. HORTON. New York: Alfred A. Knopf. 1970. Pp. 1, 422, xxxiv. \$12.50.

Gilberto Freyre is the most influential living interpreter of Brazil's past. Although sharply criticized in his own country by the younger generation, his vision of a multiracial patriarchal society remains the point of departure for those seeking to understand Portuguese America. Thanks to the pioneering publishing efforts of Alfred Knopf, Freyre is no stranger to English-speaking audiences. Since the publication of *The Masters and the Slaves* in 1946, scores of foreigners have turned to Freyre's "Proustian" prose for an explanation of Brazil's origins. They now have the third volume of his unfinished *meisterwerk*. *The Masters and the Slaves*, the first volume, ranged over the three centuries of the colonial era. *The Mansions and the Shanties* (1963), the next volume, examined the social ethos of the nineteenth-century Empire. In *Order and Progress* readers will find a fascinating, if rambling, portrait of the years bridging the late Empire and the early Republic (1870-1914). A fourth volume has been promised. It will chronicle the final chapter in the "history of Brazilian patriarchal society," a saga whose finis seems stubbornly elusive.

Fifty years ago Freyre set out to do nothing less than explain the "essential elements" of Brazilian society as it has changed through four centuries. His instinct for pungent detail and confident generalizations have earned him the rewards and obloquies reserved only for the truly influential students of national character.

Freyre's patriarchal focus proves less successful as a unifying theme in *Order and Progress* than in the earlier volumes. He nonetheless succeeds in providing an engrossing account of the intelligentsia of a traditional plantation economy caught up in the process of modernization. Methodologically, he relies almost exclusively on the analysis of "personalities as symbols," depicting values and ideologies as embodied in leading intellectuals and politicians. Little is said systematically about the size, composition, or function of institutions.

Yet Freyre's impressionistic canvas tells us much about the anguish of the small elite that agonized over their racial identity, technological backwardness, and political instability. One feels the author's enthusiastic empathy with the Brazilians who yearned to be modern while disagreeing deeply about the meaning of their past. Zealous propagandists promoted the competing ideologies of liberalism, positivism, and Catholic conservatism. Meanwhile the once prosperous Northeast continued its disastrous economic decline as the coffee-based economy of the South grew explosively. This shift of economic power was accompanied by violent dissent in the political arena. Frequent government suppression soon aroused among many of the elite a powerful nostalgia for the remarkably wide political freedoms of the parliamentary monarchy overthrown in 1889. In the last analysis this volume offers a view of Brazilian social change seen from the salon and the veranda. This failure to identify with the masses (the vast majority of the population) is hardly surprising, since Freyre has remained an authentic representative of the elite he chronicles.

The translator and publisher deserve our thanks for this carefully produced edition, shortened by several hundred well-chosen pages from the Brazilian original. *Order and Progress* merits a wide reading by all those who seek to understand the role of intellectuals in developing societies.

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JUNE E. HAHNER. *Civilian-Military Relations in Brazil, 1889-1898*. Columbia: University of South Carolina Press. 1969. Pp. xiii, 232. \$7.95.

The military's seizure of power in Brazil in 1964 emphasized that scholars knew little about the history of the Brazilian military or about its relationships with civilian society. Recently historians have turned their energies toward filling the gap. June Hahner's study is one of the first to reach print. She surveys the overthrow of the Bragança empire and the imposition of a military dictatorship under Manoel Deodoro da Fonseca and then examines in detail the struggle by powerful members of the civilian oligarchy against the continued rule of Marshal Floriano Peixoto and his followers. She correctly says that the officers who sought to dominate the government held the civilian elites in contempt. Her views of the military establishment, however, appear oversimplified as do her descriptions of their opponents. She says that the people of São Paulo state, the *Paulistas*, led the latter. But though she uses *Paulista* constantly in referring to the opposition leadership, nowhere does she explain which groups are embraced by the term.

Apparently the *Paulistas* she refers to were the entrepreneurial class whose wealth was based upon the coffee export trade. They opposed military rule not because of love for democratic government but because military rule led to factionalism in the armed forces, and that in turn caused constant struggles for power among the officers. The disastrous naval revolt of 1893 is a prime example of the kind of turmoil that would frighten capital, weaken credit abroad, and thus damage the *Paulista* economy.

Miss Hahner could well have given sharper focus to the fact that throughout the period the lines between civilians and the military were not clearly drawn, but, rather, the two groups mixed together in a series of shifting alliances of convenience. The officers felt that because they had overthrown the monarchy and created the republic, they had inherited the emperor's moderating power; the civilian elite did not disabuse them of the notion. Indeed, the constitution of 1891 specifically charged the military with the maintenance of law and order in Brazil and with guaranteeing the government's normal functioning. It also

made the military's obedience to the president contingent upon the generals' interpretation of the legality of executive orders. Such discretionary power could have been mitigated if civilians had strenuously encouraged the military toward an apolitical stance, but instead they constantly sought allies among the officers to bolster various causes. Rarely did military men enter a political fray without invitation. Rather than actors they appear as pawns in the interminable political games.

The underlying theme of the book is that the vaguely defined *Paulista* groups perceived that military rule would not bring stability, unity, or consensus of purpose and that the *Paulistas* sought to create a political system that would neutralize the national military establishment and the power of the national government. This was achieved partly through creation of strong local militias loyal to the state governments and through a series of interstate alliances among the civilian elites. Unfortunately the resulting system acknowledged the existence of the national armed forces without giving it a clearly defined role or sense of professionalism. Between 1898 and the revolution of 1964 the civilian politicians continued to draw military factions into politics whenever it suited their needs. The military did not act unilaterally but intervened only when civilian opinion supported intervention.

Miss Hahner's book raises important questions that need study. Did the *Paulista* elite's vision of prosperity based upon coffee export hold back Brazilian industrial development, and did they oppose Floriano Peixoto's rule because his ideas would have produced basic changes in the socioeconomic structure of society? Miss Hahner assumes that civilian rule equals democracy, but the civilian government that came to power in 1898 brought not democracy but continued exploitation of the Brazilian people. Despite its flaws, this is a provocative, summary account of the period that will have to be consulted for some time to come.

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Communications

TO THE EDITOR:

John P. Diggins' "Consciousness and Ideology in American History: The Burden of Daniel J. Boorstin," *AHR*, 76 (1971): 99-118, purports to show that "Boorstin's thesis [broadly, his assessment of American history as, in Boorstin's words, "doctrinally naked"] is unconvincing because his own theory of history bears so much resemblance to the very European philosophies that created the age of ideology" (pp. 100-01). This is a task worth doing. As Diggins indicates he is aware—and as Hegel and so many since him have argued—a belief in and commitment to the "real" (which is basic to Boorstin's writing) is itself a most audacious philosophical speculation, with a complex history in European thought. And laying bare the philosophical assumptions of a major historian is in any case worthwhile.

But Diggins approaches the task with a crooked thumb. It is not to such obvious sources as European positivism that he would link Boorstin's thought, but to the historical philosophies of Hegel and Marx. There is an insiders' joke here: submerged in this ideological uncovering of Boorstin is the public but not widely publicized fact of the establishment historian's one-time membership in the Communist party. A joke is fine; but, overstrained, this one misses the point. For its sake Hegel is turned into a positivist, Marx is pared down to a simple empiricist, and Boorstin, too, is distorted—a muddle of ideology is created that in the end manifests the very doctrinal nakedness Diggins had set out to disclaim. Moreover, while straining to identify Boorstin's philosophical frame of reference with those of Hegel and Marx, Diggins overlooks the one thing the three

significantly share: a particular conception of America.

Diggins' treatment of Hegel is most inept. "Boorstin and Hegel," we are told, "share a common axiological premise: since history and philosophy are contradictory, and since the empirical and the ideal are therefore one and the same, we can proceed, Hegel advised, upon the assumption that in historical study 'thought must be subordinated to what is given, to the realities of fact'" (p. 102). But that positivistic assumption that Boorstin supposedly shares with Hegel is not Hegel's at all. In the study of history, Hegel had written, "*it would seem as if* [emphasis added] Thought must be subordinate [Diggins' "subordinated" is a minor error] to what is given . . . while Philosophy dwells in the region of self-produced ideas, without reference to actuality. Approaching history thus prepossessed," Hegel continues, "Speculation might be expected to treat it as a mere passive material; and, so far from leaving it in its native truth, to force it into conformity with a tyrannous idea. . . ." But the author of the *Philosophy of History* does not approach history "prepossessed" of such a view of the relationship of fact to philosophical reflection as would make the latter appear to be an act of distortion. He presents it only to make explicit the ground for "the charge consequently brought against speculation"—a charge that is to be "confuted." His confutation is a refutation of the "prepossession" that Diggins has mistaken for Hegel's "advice": its substance is the intrusion of reason into the realm of historical fact itself (*The Philosophy of History*, tr. J. Sibree [New York, 1956], 8-9).

One suspects that Diggins leafed through the

Philosophy of History until he came on a phrase that indeed sounded like Boorstin, then grabbed it up too eagerly to notice its negative place in Hegel's argument. The second of the two quotations on which Diggins' misconception of Hegel rests, on the other hand, is the often quoted (and often misunderstood) declaration that "the rational is actual; and the actual is rational." By putting them in adjacent sentences on the assumption that they mean essentially the same thing, that both assert the primacy of the given, Diggins wrought better than he knew. For the second statement is the general form of Hegel's refutation of the first. The proclaimed identity of the rational and actual gives to reason the task of determining what is really (actually) given: whether that professor who has written many books is actually (really) a historian; whether the institution in which one teaches is actually a university; whether the person who presides over the court in which we find ourselves is actually a judge; whether the institutions that claim to govern us are really a government; whether what Diggins offers as Hegelian thought is actually Hegelian thought; and whether Hegel's identity of the rational and the actual actually implies the "fatalism" that Diggins reads into it. These are all questions of fact that must be determined by reason, and the list is endless. As in the Mencian "rectification of names," Hegel's identity of the rational and the actual makes every institution and action, every historical given, dependent upon reason for its meaning, for its being as a fact. Though in the *Philosophy of Right* it is put with its conservative face forward, this identity, far from implying fatalism, actually constitutes the ground on which social criticism can proclaim more than the willfulness of an opinion against the situations it confronts. Quite unlike Boorstin's exalted Puritans, who "make the 'is' the guide to the 'ought'" (cited by Diggins, p. 102), Hegel shifts the premise of criticism from the "ought" to the "is" by loading the latter with the dynamite of reason.

It is this transference of the premise of negation to the realm of the actual itself that Marx carried into his revolutionary theory. But to describe that theory as "based solely on the will and power to transform reality with no regard for the moral demands of the ought"—

thereby linking it to Boorstin's viewpoint (p. 104)—beclouds its essential meaning. Marxism, above all else, claimed to have discovered in the very negativeness (the "human loss") of capitalist existence the process of a further negation whereby the highest moral goal to which modern thought can aspire—a Kantian "realm of ends"—would be realized as historic fact. That Marx's commitment was first to this goal and only, therefore, to the historic engine he had discovered for its realization should be too obvious to require argument. But an instance may be cited for its pathos. "Some day," Marx said on his way back from the Hague Congress, where the International Workingmen's Association had begun to fall apart, "the workers must conquer political supremacy, in order to establish the new organization of labour. . . . If they fail to do this, they will suffer the fate of the early Christians, who neglected to overthrow the old system, and who for that reason, never had a kingdom in this world" (in Samuel Bernstein, *The First International in America* [New York, 1962], 160). That the historic upsurge of those who served as means toward others' ends might fall short of creating a realm of ends, at least the older Marx knew; that it should not was his desire.

Of course, as a "scientific" socialist claiming to have found the agency of regeneration in the structures of the vale of tears itself, Marx shared the current positivistic faith in the given—even if it was only a faith in its propensity to self-destruct. He flaunted fact, proclaimed himself "materialist," and took pains to give his arguments reference to "real" (that is, eating and working) men. But to make Marx simply an empiricist, thus to taint Boorstin with his name, and to say that "for Marx the abstract concepts of mind served mainly to delude man and thereby to preserve the social order" (p. 105) is to distort him seriously. Like Hegel, Marx certainly preferred "concrete" to "abstract" concepts; and, particularly in his early writing, to distinguish himself from Hegel, he sought to start from "real life, from the real living individuals themselves" ("German Ideology," in *Marx and Engels on Religion* [Moscow, 1955], 75). But from the declaration that "theory . . . becomes a material force as soon as it has gripped the masses," in his earliest revolutionary statement ("Contribution to the

Critique of Hegel's Philosophy of Right: Introduction," in *ibid.*, p. 50), to the intense involvement with mathematical manipulation of abstract economic categories of his later years, Marx's engagement with "concepts of mind" is overwhelming—sometimes embarrassing. When the Silesian weavers, for example, did not merely break machines ("the workers' rivals") but also burned ledgerbooks ("the title of property"), Marx saw the action as an "awareness of the essence of the proletariat" (*Werke* [Berlin, 1961], 1: 404). In the English Ten Hour Act he found "the political economy of the middle class succumb[ing] to the political economy of the working class" (in S. A. Dridzo, *Marx and the Trade Unions* [London, 1935], 111). To have this inveterate theorizer (Edmund Wilson aptly called him "the greatest rabbi in all Europe") "assuming that theoretical man is the enemy"—in order to disclose "the strangest consensus in recent American historiography" between Marxian thought and Boorstin's concern to save (in his words) "a sense for the 'seamlessness' of experience" from disruption by the contours of concept (p. 105)—is to adopt oneself the conceptual "seamlessness" of which Boorstin speaks, wherein Marx, meaning, and anything else that might have distinctness fades from view.

Boorstin, too, is distorted in this study in ideological guilt by association. To show the Marxian quality of "Boorstin's definition of the American character" Diggins compares it to Gramsci's conception of man as "a process, and precisely the process of his actions" (p. 107). Since history is the visibility of that process, the Italian Communist's statement is a valid and somewhat existentialist variant of the Hegelian (and also Marxian) conception of man as both subject and object of history with which Diggins had previously tried to identify Boorstin as "Hegelian." But Boorstin's conception of the "American character" jibes with neither Gramsci's nor Hegel's conception of man. Yes, his prototypical and ideal American is first of all a "doer," and only for doing's sake a "thinker." But if doing thereby determines consciousness and being, it is itself truly "doing" only insofar as it lets itself be guided by "reality"—that is, adapts itself to a metaphorical if not literal "bare earth" and "open air," breaks free of an "elegantly furnished"

conceptual framework, and becomes "sensitive to the unpredicted whisperings of environment" (Boorstin, *The Americans: The Colonial Experience* [Vintage ed.; New York, 1964], 149–52). This "passive material" (as Hegel would call it), a facticity prior to consciousness, not the human deed, becomes therefore the agency, the true actor, in history—in Boorstin's American history at least. Nothing so well reveals the vast difference between Boorstin's conception of America and the Hegelian-Marxian conception of history than Boorstin's statement, cited by Diggins in another context (p. 105), that the object of American doing has been "self-annihilation through mastery and adaptation"—the very antithesis of Hegel's and Marx's conceptions of man's historic self-generation. Yet there is something similar.

The admission that "these comparisons are overdrawn" (p. 107) is a weak disclaimer for an argument that has them at its heart. Diggins' misdirected joke fares somewhat better in showing a common quality in the aura that surrounds the Soviet Union and that with which Boorstin surrounds the United States, but only by revealing a vague "realism" and pragmatism (not Hegelianism, and only the most empirical variants of Marxism) as the common hagiographic term. One crucial problem Diggins entirely ignores. He fails to distinguish explicitly Boorstin's conception of America as a nation committed to the "given" from Boorstin's exaltation of the America so conceived—his own commitment to the "given." Apparently directing his attack on "Boorstin's thesis" toward both as one, Diggins actually seeks to undermine the latter by linking it to the attitudes of Hegel and Marx. The former, the truth of Boorstin's essential account, Diggins seems to accept. He writes, for example, of the "belief in 'givenness' that has led Americans . . . to the devaluation of mind and thus to the 'thingification' . . . of life" (p. 115). Were the distinction between Boorstin's telling and cheering made explicit, the former, placed within a different frame of reference (a personal ethic, some transcendent value system, or the world-historic schemata of Hegel or Marx), could significantly inform a radical and critical history. In this respect it is worth turning again to Hegel and Marx, for it happens that their particular conceptions of

American life, not their general frames of reference, resemble Boorstin's.

Behind Hegel's exuberant and often-quoted prophecy that the United States is "the land of the future, where, in the ages that lie before us, the burden of the World's History shall reveal itself," is the muted observation that, at the time of writing, America had not yet really entered the process of history, the process of the actual self-creation of human consciousness, and therefore "has no interest for us here"—that is, in the *Philosophy of History* (pp. 86–87). He briefly described a nation emerging from a situation in which "the whole attention of the inhabitants was given to labor, and the basis of their existence as a united body lay in the necessities that bind man to man" (p. 84)—what Boorstin would call "the ordinary accommodations of this world" (*Colonial Experience*, 34). The "fundamental character of the [American] community," which Hegel described as a "subjective unity" (a consensus, in our own jargon), he took to be "the endeavor of the individual after acquisition," its basic condition being unlimited space. Sharing the common European notion of American classlessness, he gave the lack of "a distinction of classes" and "the outlet of colonization constantly and widely open" as reasons for the lack of "a real State and a real Government"—and for the lack of a real history. "Had the woods of Germany been in existence," Hegel said, somewhat as Turner would later, "the French Revolution would not have occurred." America's real history would begin, he thought, "only when, as in Europe, the direct increase of agriculturists is checked, [and] the inhabitants, instead of pressing outwards to occupy the fields, press inwards upon each other" (*Philosophy of History*, 85–86).

Marx's view of America was quite similar; even the specific terms are only slightly shifted. As Hegel had seen here "the permanent example of a republican constitution. A subjective unity . . ." so Marx saw in the United States not simply "the political form of the revolution of bourgeois society," as in France, but "its conservative form of life [again consensus] . . . where, though classes already exist they have not yet become fixed, but continually change and interchange their elements in constant flux . . . and where, finally, the feverish, youthful

movement of material production, which has to make a new world its own, has left neither time nor opportunity for abolishing the old spirit world [that is, for the emergence of a new and revolutionary consciousness]" (*Eighteenth Brumaire of Louis Bonaparte* [New York, 1969], 25).

For both Hegel and Marx America and the ideologically naked confrontation with "necessity" that it embodied was the negative moment of a larger historical process—a moment that would itself be negated and raised (*Aufgehoben*) in a grander realization of human consciousness. Because of its place in that schema their attitude toward it was positive. Hegel went so far as to disparage the very citadel of the *Weltgeist* in describing America as "a land of desire for all those who are weary of the historical lumber-room of old Europe" (*Philosophy of History*, 86). Stripped of the anticipation of its dialectical self-negation, however, with even Hegel's assumption that the change in the condition of American life from one of pressing outward to one of pressing inward would be realized in a new general social consciousness still open to question, the American experience, in Hegelian and Marxian terms, is simply negative: an erosion of the complexity of the mind in the single-minded rape of "bare earth"; the epitome, the "conservative form of life," of what Marx saw as the "loss of man" in the capitalist enterprises through which men sought to determine their own condition—or, as Boorstin put it, "self-annihilation through mastery and adaptation."

If Boorstin ever operated within a Hegelian-Marxian frame of reference, he has abandoned it. His is simply (or complexly) pragmatic. Yet he sees essentially the same America that Marx and Hegel saw—some hundred-odd years later. The account with which he opens his "reinterpretation" of American history—the New England Puritans, letting the sophisticated ideological structures they had brought from Europe crumble, guide the development of American existence, while the Pennsylvania Quakers, to maintain ideological commitments, forswear power—can stand as a fair paradigm (its meaning somewhat sharpened with a de-idealized Quaker as commander in chief) of the American experience seen as essentially an erosion of mind. It is a pattern that repeats

again and again. Without the Hegelian-Marxian frame of reference, which includes the anticipation of a redeeming *Aufhebung*, and without a set of transcendent values, Boorstin hails the "is" as the "ought." Here one may take issue. One may cheer the Quakers of 1756, for example, without denying that they lost and put down the Puritans without denying that they won. Ultimately it is not so much who or what Boorstin cheers as what he relates that matters. And what he tells, from other points of view, is a terrible tale. It is that which Allen Ginsberg intended when he taunted (in the same dreary decade in which Boorstin began his reinterpretation): "America I still haven't told you what you did to Uncle Max after he came over from Russia." What matter that he tells it, so to speak, in Uncle Max's words?

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PROFESSOR DIGGINS REPLIES:

Mr. Berland's informed and sensitive letter reflects the sentiments of others who have written me regarding the Boorstin piece. They, too, wonder why I was not content merely to describe Boorstin as a positivist or empiricist and to launch my analysis from that convenient springboard. Apparently for some it is an ideological embarrassment to bring to light the subtle, stunted Hegelian and Marxist implications in Boorstin's writings. Now that Hegel has been rehabilitated by the Frankfurt school, no one wishes to be reminded that Americans like Whitman and Royce saw in German idealism a philosophy of celebration and reconciliation. Whitman's metaphysical spider, endlessly spinning its essence "out of itself," must not be allowed to ensnare Hegel's owl in a web of perfect contradictions and thereby domesticate the dialectic, the last myth of Marxism.

My principal disagreement with Boorstin had its origin in a revulsion against the naturalistic fallacy inherent in the concept of "givenness"—a concept that seemed to me to justify more than it explained the American past. The possibility that Boorstin's writings harbored illicit Hegelian and Marxist conceptions was a matter I took up mainly to demonstrate that the alleged distinction between Amer-

ican pragmatism and European ideology is questionable if not false. The *AHR* article represents the second part of a two-part critique. Perhaps the first part, wherein I deal with Boorstin's actual historical interpretations, may fulfill Berland's request for a "personal ethic." (See "The Perils of Naturalism: Some Reflections on Daniel J. Boorstin's Approach to American History," *American Quarterly*, 23 [1971]: 153-80.)

There is already too much Hegel in the Boorstin article, and I fear that the following reply to Berland will merely affirm Max Eastman's remark that "Hegelianism is like a mental disease—you cannot know what it is until you get it, and then you can't know because you've got it" (*Marx and Lenin: The Science of Revolution* [New York, 1927], 22). Berland believes that Hegel and Boorstin cannot be mutually "tainted" because, while the latter's frame of reference is only "simply (or complexly) pragmatic," the former brought the "intrusion of reason into the realm of historical fact itself." Boorstin's ecstatic deference to the "beautifully soothing" historical fact contains, I submit, similar metaphysical implications, and the qualifying utterance of Hegel that Berland quotes ("it would seem as if . . .") does not in the least detract from the common assumptions shared by Hegel and Boorstin. After explaining that "the task of philosophy *seems* [emphasis added] to be in contradiction to this objective [of history]," Hegel goes on to assert: "But we have to take the latter as it is. We must proceed historically, empirically" ("Selections from *The Philosophy of History*," in Carl J. Friedrich, ed., *The Philosophy of Hegel* [New York, 1953], 3-5). Hegel and Boorstin can assume to take the empirical data of the past as it is because they incorporate philosophy into history and read into fact the transforming power of reason or nature. When I described Boorstin's interpretation of the American past as a "Hegelian metaphysics of history" I was referring to historicism; not to the positivist notion of historicism, but to the nineteenth-century Teutonic idea that the basic distinction between philosophical and historical questions cannot be maintained. "In America," Boorstin declared, "what would liberate men was not the opportunity to combat ancient and erroneous philosophical systems by modern ones,

but the opportunity to bring *all philosophy* [emphasis added] into the skeptical and earthy arena of daily life" (*The Americans: The Colonial Experience* [Vintage ed.; New York, 1958], 154). The irony is that both Hegel and Boorstin are "prepossessed" with the very philosophical reflection they claim to deny. As philosopher-historians Hegel and Boorstin reject the subject-object and fact-value dualisms, the former by investing reason with the role of mediation, the latter by investing matter with the role of mind. Hence Hegel can confute "speculation" in world history and Boorstin can confute "theory" in American history only because of their conviction that subjective value grows out of the objectively given through the dialectical interplay of mind and nature.

Berland suspects me of having "leafed through" (with a "crooked thumb"!) Hegel's *Philosophy of History* and of having put Hegel's famous dictum ("the rational is actual; and the actual is rational") "in adjacent sentences on the assumption that they mean essentially the same thing." One suspects that Berland's modern exegesis of *The Philosophy of History* derives from Herbert Marcuse's fascinating redemptive interpretation of Hegel. This is one way to keep the faith. Anyone familiar with Hegel will nevertheless recognize that the dictum appears in the original exactly as I quoted it. Nor did I assume that the two statements mean the same thing. Inspired by the logic of self-contradiction, Hegel's dictum poses a riddle that allegedly contains its own resolution: the presumed power of reason to negate the immediate actual in light of the ultimate rational. Berland then ticks off a list of questions and naively asserts that "these are all questions of fact that must be determined by reason." Yes and no. Some are factual and merely a matter of truth or falsehood, what might be called "is" and "is not" questions. Others may be regarded as moral and requiring an a priori understanding of the "ought" and "ought not." Blurring this distinction, Hegel allowed reason to inform him that the existing Prussian state was a real government. In the same way Berland blurs the distinction when he tells us that reason will determine "whether that professor who has written many books is actually (really) a historian" (which is perhaps the academic version of the law of the transformation of

quantity into quality). Those aware of the crucial distinction between fact and norm will find cold comfort in the magical powers of Hegel's celebrated reason. For what Berland fails to acknowledge is the tortuous ambiguity arising from the active and the passive counter-currents that flow through Hegel's philosophy like a magnetic field. If the rational actualizes itself as history, rational reform or revolution may triumph; but if the real is the rational then existing institutions may be endowed with historical legitimacy. The ontological premises in Hegel's "bud" and "blossom" principle and in Boorstin's "exoskeleton" concept are open to either interpretation. Finally, Berland is convinced that Boorstin is not to be compared to Hegel because the German philosopher "shifts the premise of criticism from the 'ought' to the 'is' by loading the latter with the dynamite of reason." This is a lame distinction, for Boorstin also shifts the focus of historical criticism and understanding, in a quite similar way, by loading the American environment with the self-active dynamism of "givenness." In Hegel's "is" lies the paradoxical power of negation and reintegration; in Boorstin's "is" lies the mysterious "unpredicted whisperings of the environment" and the "mystical power of our landscape." True, their intentions may be different, but philosophically the consequences are the same: the object (history) is conceived subjectively, and reason or nature (the "is") fulfills the essential function of spirit (the "ought").

That the difference between intentions and consequences is an empty distinction (and also un-Marxian) may be better illustrated when we turn to the question of ethics. "Marxism, above all else," Berland declares, "claimed to have discovered in the very negativeness (the 'human loss') of capitalist existence the process of a further negation whereby the highest moral goal to which modern thought can aspire—a Kantian 'realm of ends'—would be realized as historic fact." Marx would be surprised to learn that he had wanted modern thought to aspire to Kantian idealism rather than go beyond it. Doubtless Marx nobly desired to realize a state of moral existence. But Berland confuses what Marx desired with what he described and discovered—or thought he had discovered. And exactly what Marx discovered

Berland does not explain, perhaps because Marx himself could not account for the primal cause of alienation, choosing instead to leave the problem where he found it: "It is the riddle of history solved and knows itself as this solution." Equating a condition with an explanation, Marx prescribed a solution without a causal connection: "The overcoming (*Aufhebung*) or self-alienation follows the same course as self-alienation" ("Economic and Philosophic Manuscripts [1844]," in Loyd Easton and Kurt Guddat, eds., *Writings of the Young Marx on Philosophy and Society* [New York, 1967], 301, 304). It's as though Hume never happened.

The unresolved contradiction need not trouble Berland. To me it seems like the triumph of wish over intellect, for as I hope to point out in a forthcoming study of Thoreau and Marx, there may be an ironic rub within Marx's unexplained "riddle." In making work rather than thought the center of man's being, did not Marx absorb the ontological principles of Protestantism and capitalism? "By producing food," Marx asserted, "man indirectly produces his material life itself," and thus what men are "coincides with *what* they produce . . . and *how* they produce" (Marx and Engels, "The German Ideology," in Easton and Guddat, eds., *Writings of the Young Marx*, 409). With this definition of human nature man is transformed into an *animal laborans*, a transformation wrought by Locke and Adam Smith as well as by Marx. Moreover, by focusing solely upon the "what" and the "how" and avoiding completely the question "why," Marx limited his analysis to the processes of man's activities and not their ultimate purpose. It is difficult to see how Berland's Kantian distinction between means and ends can be preserved if man's essence is regarded as the process of his activities. I believe this distinction is lost sight of in the Marxist as well as the capitalist value systems, both of which appear to treat man as a means, judging him by what he does and not by what he is. And the political consequences of this naturalization of man are everywhere to be seen. Now that the *homo faber* has become the ideal, all other possible modes of being are suspect and subversive. Fidel Castro rounds up Cuba's long-haired youths and ships them off to labor camps where, together with "the lazy ones," he plans to "re-educate those people

through work" (Castro's speech of July 26, 1970, in the *New York Review of Books*, Sept. 24, 1970; Joseph Clark, "Thus Spake Fidel Castro," *Dissent*, 17 [1970]: 38-56). The Nixon administration moves to cut off food stamps to the hippies. The comrade and the citizen must work, whether work be an inherent creativity or an inherited curse. The critical sting of Thoreau's thought has yet to be felt; as Emerson remarked, we are all embarrassed by the satire of his presence.

If it were merely a matter of waiting for the realm of freedom to overcome the realm of necessity, one might assert the will to believe. But Berland's faith in negation by reason requires more than a suspension of disbelief. Marx's account of what Berland so poignantly calls "human loss" and regeneration is about as convincing as Milton's account of the Fortunate Fall and the Resurrection. One difficulty is that Marx's equivocal attitudes toward the meaning of labor betray what Hannah Arendt has rightly described as "a fundamental contradiction which runs like a red thread through the whole of Marx's thought" (*The Human Condition* [New York, 1959], 90). Once the source of value is located in the labor process, the concept of moral obligation—the realm of ends—becomes not a possible transcendent ideal but an actual physical activity that is simply "produced." Convinced that man could know only that which he produced, and thus somehow assuming that productive man would be moral man, Marx could describe the consummation of communism with the same flourish of naturalistic lyricism that characterizes Boorstin's idea of consensus: "This communism as completed naturalism is humanism, as completed humanism it is naturalism. It is the genuine resolution of the antagonism between man and nature and between man and man; it is the true resolution of the conflict between existence and essence, objectification and self-affirmation, freedom and necessity, individual and species" ("Economic and Philosophic Manuscripts," 304). In Boorstin's mind this state of pre-alienated wholeness, this philosophical wonderland where all existential contradictions have been resolved, has already been realized in the American past: "Nothing could be more absurd than to try to make of isolated utopian communities in American history any-

thing like a great tradition of utopianism in the main stream of our thought. Some of our historians, desperately searching for articulateness and for ties with the European tradition of socialism, have attempted just this. They miss the essential point that the whole American experience has been utopian" (*The Genius of American Politics* [Phoenix ed.; Chicago, 1958], 173-74).

As I indicated in my *American Quarterly* article, Boorstin himself misses the point. The realm of ideals and ends has not been realized as "historical fact," and one does not have to be "desperately searching for articulateness" to be aware of the painful gap between the ideal and the real. Indeed it is the nature of ethical existence that the norm can never be realized as fact. Yet Marx and Boorstin (and apparently Berland), by projecting their own subjective desires upon the objective movement of history, assume that it can. Marx denied the possibility of conflict in the future state of communism. Boorstin, reversing Marx's advice that we draw our "poetry" from the future and not from the past, denies conflict in the "utopian" American experience. Both conclude there is no need for moral ideas and ethics in the study of history. Even Hegel—who respected "conscience," recognized the difference between the private and the public realm, and wanted to preserve human diversity within unity ("particularity")—did not go this far (whether forward or backward) in consensualizing history and denying its dramatic moral tension. But with Marx and Boorstin value loses its imperative force and normative meaning. And yet, when all is said, Berland is absolutely right in stressing the nobility and pathos of Marx's vision. Marx warned us that we cannot abolish philosophy until it has been realized in history. Boorstin would have us believe that it has been realized and that America's "unique" past demonstrates the end of "pre-history." Marx may have asked us to aspire to the impossible, but Boorstin asks us to accept the inevitable.

Berland claims that Marx could never have assumed that "theoretical man is the enemy" because he himself was an "inveterate theorizer." Here Berland completely ignores the distinction I made between the two theories of theory: moral theory grasped in contempla-

tion, practical theory realized in action. Berland also misses the irony. Marx sitting in the British Museum and Boorstin in the Smithsonian Institution are clearly men of thought rather than action. Yet both have used ideas and theory not only to "confute" theoretical ideas, but to demonstrate that history shuns the thinking man and embraces the doer and maker. If history is primarily the study of power and success, then Marx and Boorstin are essentially correct, albeit for different reasons. My article can be read as, among other things, a plea for intellectual history, which is more often the study of beautiful losers. In this graveyard of the historically damned Berland may find Trotsky along with the Quakers.

One senses Berland's ambivalence when he turns to the problem of consciousness. "Nothing so well reveals the vast difference between Boorstin's conception of America and the Hegelian-Marxian conception of history than Boorstin's statement, cited by Diggins in another context . . . that the object of American doing has been 'self-annihilation through mastery and adaptation'—the very antithesis of Hegel's and Marx's conceptions of man's historic self-generation. Yet there is something similar." This "something similar" is nothing less than the metaphysical foundations of an industrial ethos that embraces Marxism as well as capitalism: that knowing and doing are one in the same, that self-knowledge is merely a matter of producing, and that man grasps the essence of his self in the objects he produces even though he may destroy these objects by consuming them and pollute nature by "mastering" it. (Thoreau desired to "know beans" and not merely to "eat" them.) Berland may continue to believe that man rises to consciousness by acting upon nature through "industry," which Marx called "the *open book of man's essential powers*" ("Economic and Philosophic Manuscripts," 310). So does Boorstin and all those who believe that man finds himself in deed rather than in thought. The paradox in Boorstin's activist idea of self-realization through "self-annihilation" is as mystifying as the dialectic itself; as Berland senses, it comes uncomfortably close to Hegel's and Marx's conception of historical salvation as "the negation of the negation."

The last several paragraphs of Berland's perceptive letter seem more a confirmation than a criticism of my argument. He tells us that Boorstin "sees essentially the same America that Marx and Hegel saw" and that therefore "Diggins overlooks the one thing the three significantly share: a particular conception of America." I had been aware of Hegel's and Marx's scattered thoughts on America, but I found them too fragmentary and nebulous. It is curious that several years ago, in a review of Ray Allen Billington's *The Frontier Heritage* (*The American West Review*, 1 [1967]: 15, 23), I used a critique similar to Berland's to chide historians of the American West. At the time my views were influenced more by D. H. Lawrence's familiar observation that America has been an "anti-life," a "false dawn" of endless flight where the "real day" cannot begin until Americans bethink themselves and know what they want to "be." But I was also intrigued by the different perspectives of Hegel and Frederick Jackson Turner, one believing that American consciousness would arise only with the end of the frontier, the other lamenting the passing of the frontier as the fountain of democratic values. In criticizing contemporary Turnerites for praising the restless mobility and compulsive individualism of the American character—for which I was blasted in a subsequent issue—I questioned the "fevered American Dream" and quoted Tennessee Williams' remark that we are "attempting to find in motion what was lost in space."

Nowhere in the article did I say that Boorstin is a Hegelian or a Marxist. I suggested, rather, that there is a "strange" resemblance in their approach to history based upon common axiological, epistemological, and ontological assumptions. When asked why he had become a Communist Boorstin replied: "It was primarily an interest in a theory of history—the materialist interpretation of history—together with the feeling that these people were at the same time standing up for humanitarian causes and fighting anti-Semitism in Nazi Germany, for example" (U.S. Congress, House, Committee on Un-American Activities, *Communist Methods of Infiltration (Education)*, Hearings, 83d. Cong., 1st. sess., 1953, pt. 1: 57).

It is likely that Boorstin has recently repudiated "the materialist interpretation of history." Two years ago on a William Buckley "Firing Line" television broadcast Boorstin agreed with Buckley that the "desired" and the "desirable" must be kept distinct, a distinction that undermines Boorstin's whole concept of "givenness." But Berland's flippant generalization is too simple and almost contradictory. He desperately wants to believe that Boorstin must have "abandoned" Hegel and Marx when he embarked upon his reinterpretation of the American past: yet Berland is at a loss to explain how all three could have such strikingly similar conceptions of American history. We must remember that Boorstin, despite his depreciation of ideas and theory, is really writing philosophical history to justify America as an expression of the dialectical resolution of mind against nature and man against man. Boorstin's work can be read as elegant narrative history; it can also be read ideologically as part of that dubiously "unique" legacy of the "twice-born" Old Left: a Marxist solution without a Marxist problem, a social history without class struggle, an intellectual history without "discontent" intellectuals. If the proletariat and the intelligentsia failed Boorstin, the dialectic has saved Berland.

Which brings us, then, to the crux. Although I cannot share Berland's faith in Hegelianism, I completely agree with his statement that the America that emerges from Boorstin's history is "stripped of the anticipation of its dialectical self-negation." It is for this reason that I repeatedly stressed that Boorstin offers us an "Americanized" version of Hegel the purpose of which is emphatically "not negation but affirmation." I thought I made my position clear when I stated that Boorstin's philosophy of history denies the "quest for transcendence." Indeed the very absence of *Aufgehoben* is precisely what I meant by the phrase "truncated Hegelianism."

After accusing me of engaging in a "misdirected joke," Berland concludes on a note of Ginsbergian *Angst*. What matter that Boorstin tells his story in "Uncle Max's words"; it "is a terrible tale." Believe me, Oscar Berland, no joke was intended. The genius and the terror of Boorstin's tale is its one-dimensional truth.

Surely a Hegelian can appreciate the cunning of history as he awaits the miraculous unfolding of reason. It's not that my thumb is crooked. My fingers are crossed.

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TO THE EDITOR:

George L. Yaney's review of A. M. Anfimov's *Krupnoe pomeshchich'e khoziaistvo evropeiskoi Rossii (Konets XIX-nachalo XX veke)* (Large-Scale Landlord Economy in European Russia [End of the 19th to the Beginning of the 20th Century]) (*AHR*, 76 [1971]: 170-71) is not only unfair in its indication of the work's content and import, but it is also incorrect on a number of factual details. Clearly the major purpose of Anfimov's work is to demonstrate the predominance of precapitalist forms of management and operation in Russian *pomeshchik* (landlord) agriculture even as late as 1917 as well as the almost total but universally detrimental interdependence of *pomeshchik* and peasant agriculture. (See Anfimov, pp. 47, 75, 77, 79, 88, 93, 118-19, 130-31, and 133-34, for example; see also summaries, pp. 177-88 and 365-67; and *passim*.)

Within this framework Anfimov has succeeded in describing and documenting to a hitherto unprecedented degree the huge variety of ways in which both the capitalist and precapitalist features of Russian agriculture were intermeshed and the kinds of conditions that led to such a state of affairs. Unlike other modernizing societies in which modern and traditional forms of agriculture existed side by side, in Russia the individual *pomeshchik*'s estate, no matter how advanced, always remained dependent on the exploitation of the surrounding peasants and their economies—even when it was in a position to break free of such dependence. (See for example Anfimov, pp. 152-54.) And this parasitism, in combination with the changes in the traditional socioeconomic relations between the *pomeshchik* and peasant that accompanied the slowly advancing modernization of the landlord economies, resulted in an ever-increasing cycle of peasant impoverishment—a process that was vividly demonstrated on the left bank of the Ukraine in the period preceding the disorders there in

1902. Furthermore, these features were even characteristic of such relatively economically advanced regions as the Baltic, the Southwest, the Southern Steppe, and the Kuban (*ibid.*, pp. 62, 158-76, for example). Similarly, on the social level, it is demonstrated that such phenomena were not in the least confined to the "reactionary" *pomeshchiki* but were equally true of the "progressive" *kupechestvo* (merchandise) and other landowning groups as well as of large-scale nonpeasant entrepreneurial renters (*ibid.*, pp. 87, 116-17, 144-45), while the most backward properties of all were generally the "largest"—the so-called latifundia (*ibid.*, pp. 26-27, 47, *passim*).

Anfimov's book is also methodologically significant, for his analysis is not concerned with the means alone by which the *pomeshchik* conducted his own economy but also with the form in which he utilized the remainder of his property—the area of which often exceeded that of the demesne itself. The overall concern, then, is with the totality of *pomeshchik* land utilization and the forms of conducting the economy as opposed to the more traditional presentation of dazzling arrays of statistical materials on "trends" in the application of agro-technical and capital improvements and so on. (See *ibid.*, pp. 94, 116, and 367).

Let me turn to Professor Yaney's characterization of the Anfimov work as yet another that follows the party line and to Yaney's injudicious references to Lenin. It is important to note that, while Lenin may have asserted that "Russian agriculture could not become capitalist as long as big estates continued to impose the old serf relationships on the Russian countryside," he also repeatedly claimed that it could become capitalist as a result of the Stolypin reforms that, following the so-called Prussian path, specifically preserved the "big estates" and as a consequence also "the old serf relationships." (See for example V. I. Lenin, *Collected Works* [4th ed.; Moscow, 1960-], 13: 243, 370, 412-13; 15: 42-45, 135, 140, among other such references.) Professor Yaney ignores the fact that Lenin's statements can be used for two purposes, depending on the nature of his adversary: to demonstrate either the high degree of development of capitalism in the economy or the persistence of traditional and semifeudal features. Indeed these two Lenins

were the source of a major historiographical controversy in the Soviet Union between the late S. M. Dubrovskii and Anfimov. (See S. M. Dubrovskii *et al.*, eds., *Osobennosti agrarnogo stroia Rossii v period imperializma* [Features of the agrarian order of Russia in the period of imperialism] [Moscow, 1962], 5-44, 64-85.) It is, therefore, quite wrong to see Anfimov's work as merely one more that follows the traditional path of interpretation. On the contrary, it is a major revisionist work in this respect.

On a point of interpretative criticism one should note that in suggesting that the landed estates did not impose backwardness on themselves and that the peasants were responsible for this state of affairs, Professor Yaney misses the major point, which is that the post-1861 social regime, as the pre-1861 social regime, was in large measure responsible for the peasants' general economic and social backwardness. By ignoring the origins of the peasants' influence on *pomeshchik* agriculture, Professor Yaney makes a grave error—though this is not to deny “the difficulty of introducing modern agricultural methods to peasants.” The question the historian should ask is why this was so difficult.

Under serfdom the *pomeshchik's* preference for social stability over a rationalized economy acted as a powerful hindrance to the modernization of both *pomeshchik* and peasant agriculture. Similarly, because the concerns of the Emancipation's framers were primarily those of accomplishing the liberation of the peasantry with the preservation of social stability, the statute's terms resulted in the perpetuation of the content of serfdom albeit clothed in new legal forms. While the *pomeshchik*, however, is usually considered to be the necessary conduit for improvements in peasant agriculture before the Emancipation, after 1861 the peasant himself has generally been burdened with this responsibility—at least by many historians and contemporary observers. That the peasant was as incapable of bearing this burden after 1861 as he was before, however, was not so much a result of his “darkness” and his personal responsibility as of the consequences of the governmentally determined conditions of the Emancipation, which continued to hold the peasant in a state of social and economic subjection. At the same time the peasant was prevented from initiating improvements in his own behalf by

his crushing poverty and by the total lack of any surplus input that could help raise productivity let alone absorb the reduction in harvest that would inevitably follow even a change to an improved system of crop rotation. (See Michael Confino, *Systèmes agraires et progrès agricole* [The Hague, 1969], 15, 275-340, 360-69, for an excellent discussion of many of these problems.) Thus after the Emancipation as before the peasant had his revenge on the *pomeshchik* by holding the latter's economy to the level of his own. (See Michael Confino, *Domaines et seigneurs en Russie* [Paris, 1963], 104.) But, as I have attempted to make clear, the determining role the peasant played on the level of *pomeshchik* agriculture was the second, not the first, turn of the wheel.

Let me mention finally two points of factual error in Professor Yaney's review: (1) Anfimov's references to A. N. Engelgardt on pages 179 and 195 do not mention the latter's condemnation of estate owners. (2) The reference to Anfimov's lack of relevance in the comparison of legal institutions in German peasant society with irresponsible (*dikie*) Russian landowners on pages 360-62 is untraceable. On page 361 Anfimov does refer to the existence of *zakon* (law) as a positive factor in the efforts of the German peasant to defend his rights vis-à-vis the landowner, but he makes no reference to *dikie pomeshchiki*.

DAVID A. J. MACEY
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PROFESSOR YANEY REPLIES:

My thanks to Mr. Macey for his interest and for his additions and corrections to my review. There is certainly no disagreement between us regarding the fact that Anfimov's book is a very valuable one, or that Anfimov does make his point about the continuance of backward agriculture on large estates in Russia prior to the Revolution. Mr. Macey says that “the major purpose of Anfimov's work is to demonstrate the predominance of precapitalist forms of management and operation in Russian *pomeshchik* (landlord) agriculture even as late as 1917 as well as the almost total but universally detrimental interdependence of *pomeshchik* and peasant agriculture.” I thought I said more or

less the same thing in my review, but if I didn't make it clear Mr. Macey has done a service to scholarship by repeating the message.

On the other hand, I do not share Mr. Macey's interest in Lenin's polemics on agriculture, and even if I did I would be hard put to justify the inclusion of an elaborate discussion of them in a brief book review. As I see it the historiographical issue is as follows: A pamphleteer of the early 1900s conceived of two possible directions for the development of Russian agriculture. Dubrovskii found one of these suitable as a basis for studying peasant villages and government policies; Anfimov chose the other as a basis for discussing landed estates. I see no major revision in this debate. It is an important event when an occasional Soviet scholar finds it possible to publish a book based on his own analyses rather than Lenin's pamphlets, but Anfimov relies as heavily on Lenin as Dubrovskii does. Indeed, both of these renowned scholars make a good deal more of Leninist exegesis than many of their more daring colleagues, and in my opinion, therefore, neither of them is especially revisionist. Mr. Macey sees things differently. That is his privilege, and I am glad he has had the opportunity to state his view.

Mr. Macey sometimes goes beyond Anfimov's claims. Anfimov would not insist—not characteristically at any rate—that the interdependence of *pomeshchik* and peasant agriculture was “universally” detrimental, nor would he argue that the individual *pomeshchik*'s estate “always” remained dependent on the exploitation of the surrounding peasants. As he frequently states, there are too many gaps in his data to allow for absolute assertions of this sort.

When Mr. Macey echoes Anfimov's general interpretation of the causal nature of government policy and *pomeshchik* “parasitism,” however, he is quite faithful to the original. Moreover, he seems to share Anfimov's supreme self-confidence that his interpretation is the only possible one. In my opinion this sense of assurance does no credit either to Anfimov or his defender. Perhaps I am only revealing my own obtuseness, but I am compelled to say that the more I read about Russian agriculture and the government's rural policy in the last century of the tsarist regime, the less assurance I feel about any statements of cause and effect

(or good and evil) and the more suspicious I become of those who make them glibly.

Let me repeat that in view of the enormous difficulty of saying anything coherent about the Russian countryside, Anfimov does very well at it on the whole. On the other hand, he frequently stretches evidence or leaves it out, usually in order to sustain his close connection with Lenin. Take, for example, that tired old issue, the shortage of peasant land. In one section Anfimov gives figures to indicate that landed estates in Russia had a disproportionate share of the land (pp. 22–31), and he compares these figures with their equivalents in Germany. The figures embody a distortion, however, in that they do not distinguish arable land from nonarable (this is also Lenin's oversight). On the basis of these gross area figures Anfimov asserts that there was plenty of unused land in Russia (p. 22), that landed estates held a considerable portion of it (pp. 22–23), and that Russian landed estates held an even more disproportionate share of the land than did their German counterparts (pp. 26–27). But Anfimov's figures in this section do not show how much of the estates' land was arable; thus, they do not support his implication that Russia had plenty of land for the peasants. His point that some landed estates were large is well taken, but when he uses gross area measurements to imply that big estates were still the chief stumbling block to agricultural development in the 1900s he loses his credibility. It is well known that by 1914 only seven to eight per cent of Russia's arable land was on landed estates.

This example is not directly germane to Mr. Macey's remarks on the sweep of agricultural history from 1861 to 1917. I only cite it as one example of those deficiencies in Anfimov's work that I had in mind when I suggested that his information was not well organized.

I shall not venture to argue the general question of causation in Russian agricultural development. Mr. Macey has his impression; I have mine. If Mr. Macey has anything more convincing than impressions his work will constitute a major revision—indeed, a revolution—in the field. He has my best wishes. I hope, however, that he will ultimately rise above outworn old controversies between such moralistic fantasies as governmental “oppression,”

gentry "parasitism," and peasant "darkness." Class antagonism was a fact of life in those days, but scholars have more to do now than to sustain dead conflicts. They should try, rather, to comprehend the bases for these conflicts in the difficulties of rural development.

With regard to my factual errors, I recall seeing the word *dikie* in connection with Russian landholders, but I confess that I cannot find it now. However, references to the irresponsibility of the landholders are frequent in Anfimov (for example, pp. 294, 340, 344), and he does draw the contrast I referred to in my review. So far as I can see, Mr. Macey is correct in pointing out that Engelgardt's condemnation of landholders does not appear in Anfimov. I must have had in mind the quotation (p. 181) from A. P. Mertvago, Engelgardt's follower, which reads much like Engelgardt's own words.

GEORGE L. YANEY
University of Maryland

TO THE EDITOR:

Only with diffidence do historians in a military academy venture to comment on Professor Marder's encyclopedic knowledge of British naval history. But, belatedly because of the postal strike, we would like to comment on the professor's article, "The Royal Navy and the Ethiopian Crisis of 1935-36" (*AHR*, 75 [1969-70]: 1327-56), and, more particularly, to suggest lines of research that we feel are crucial to an understanding of the naval events of the 1930s.

We cannot help wondering whether the Admiralty's reluctance to go to war over Ethiopia was not more a reflection of the hesitation of their political masters than anything else, of an anxiety not to be labeled as a war party, or of a subconscious feeling that a war begun with such hesitation might not fare well, since the reasons that were advanced, according to Professor Marder, seem more akin to rationalizations that justified a predetermined desire not to fight rather than to justifications of inactivity.

The purely naval arguments seem unduly cautious. Italy's two unmodernized 12.6-inch-gunned battleships, a 21-knot, 4,000-ton seaplane carrier (hardly meriting the Admiralty description as an aircraft carrier and known to be

employed ferrying aircraft to Eritrea), even the modern cruiser and destroyer units, would surely not have been able to inflict the damage apprehended. The threat of the *Regia Aeronautica* and the ammunition shortage were more serious but not insurmountable if ministerial action had ensured rapid production and delivery and if time had been gained by diplomacy. The French were by no means indispensable, and the value of resolution could have been argued to be likely to restrain Germany, who was in a very early stage of rearmament. On land a war against Italy might have been protracted, at sea it could hardly have been so—and the fleet would have been available for the Far East. And, in fact, it could have been more cogently argued that resolution was essential; if trouble developed in the Far East, the elimination of Italy and the securing of the Mediterranean line of communication were prerequisites for action east of Aden.

Research on this period appears to need to concentrate on two issues. The first of these is what happened in the live confrontations that took place. The Italian Navy's submarines were operating close patrols off Malta and Alexandria; on occasions it would seem surprised to the surface by the navy's protective patrols. It would be interesting to know the Italian plans in the event of war; some form of Pearl Harbor seems to have been contemplated, but their difficulties must have been formidable.

Second, the crisis raises an important general question—alas very difficult to answer, as the official papers are unlikely ever to be opened—on the navy in the interwar period, namely its intelligence system. In contrast to its accurate knowledge of the German and Italian navies an amateur vagueness pervades the navy's appreciation of the Japanese, although until the mid-1930s they were considered the most probable foe. The contrast is so marked as to lead one to speculate whether in the 1920s some division of Intelligence observation work had not been made with the U.S. Navy from which, in the long run, the Royal Navy did not benefit. At the time of this crisis, for example, at least one-half of Japan's battle fleet of ten ships and one of her two big carriers were commencing drastic modernization, and her two latest cruisers were proving unsta-

ble. But the Admiralty did not appear to know this and feared that the maximum available British force of seven battleships, two battle cruisers, and two or three aircraft carriers would prove inadequate. Even if the Italians had been able to inflict some damage on the Royal Navy's major units the Japanese were hardly in a position to profit. This lack of basic knowledge, if such it was, persisted; on March 1, 1939, an Admiralty spokesman told the Commons that he had no knowledge of Japanese construction of 45,000-ton battleships—the *Yamato* and the *Musashi*, both 65,000 tons, had by then been under construction for over a year. The officially assisted *Jane's Fighting Ships* had no knowledge of these ships as late as 1941, by which time they were at sea. In the war Churchill laid down instructions directing combinations of inferior British capital ships necessary to engage the *Tirpitz*; no such instructions appear to have been issued, even in battleship terms, to the hapless Force Z, which arrived at Singapore when the even more powerful *Yamato* was on sea trials.

Perhaps through Intelligence, perhaps through a prudent "belt and braces" policy, United States ships were better designed to counter their Japanese contemporaries—even allowing for complications in Europe from 1936 onward that had their influence on British priorities. Even admitting the difficulties of observation there can be few better examples of an Intelligence failure and its consequences than that of the Royal Navy and the Far East in this period.

ANTHONY CLAYTON
H. P. WILLMOTT
*Royal Military Academy,
Sandhurst, Camberley*

PROFESSOR MARDER REPLIES:

The government's "hesitation" had no bearing on the "reluctance" of the Admiralty to go to war over Ethiopia. At least I have not seen a shred of evidence to support such an interpretation. Moreover, this would have been entirely contrary to the outlook of the Admiralty in Chatfield's time. It was, as I tried to bring out, the other way around: Admiralty doubts reinforcing and, in the final analysis, determining the timid policy of the govern-

ment. It will come as news to Lord Chatfield, wherever he may be, to learn that the Board of Admiralty and the Naval Staff were simply rationalizing a "predetermined desire not to fight."

"The purely naval arguments seem unduly cautious." So they do, only we must remember that the Admiralty was not in full possession of the facts (precise strength and combat worth, for example) about the Italian and Japanese navies. And there was uncertainty about the capabilities of air power in sea warfare. It was left for the experience of the war to dispel doubts on both points. Put shortly, the Admiralty should be judged for what they knew, or thought they knew, at the time. The villain, if one can speak of a villain, was the faulty naval intelligence during these years. Here, too, there is an important extenuating factor. In Donald McLachlan's words (*Room 39: Intelligence in Action, 1939-45* [London, 1968], 54): "While Chatfield was toiling to build up the Navy with inadequate funds, it was not to be expected that NID would be treated as anything but the Cinderella of the Naval Staff." I am in complete agreement with Messrs. Clayton and Willmott that research is needed on the navy's interwar intelligence system. The NID papers are presently (and, one fears, for some time) out of bounds; but much could be learned from a thorough study of the CID papers and especially the minutes and memorandums of the Chiefs of Staff Sub-Committee, now available in the PRO. Also, Vice-Admiral Sir James Troup, DNI, 1935-39, is alive, as are Rear-Admiral J. H. Godfrey, who succeeded him in February 1939, and Admiral Sir William James, the deputy chief of navy staff, 1935-38. There is a job to be done here!

As regards Italian plans in the event of war with Britain, we may expect that Professor George Baer, who is preparing a monograph on the Ethiopian War, will oblige us in due course.

ARTHUR MARDER
University of California, Irvine

TO THE EDITOR:

Others beside myself must have noted with surprise Howard Jay Graham's strange reference,

in his survey of Leon Friedman and Fred L. Israel, eds., *Justices of the United States Supreme Court* (AHR, 75 [1969-70]: 2127-30) to his own "thirty years' unrequited labor." Most historians would be ashamed to set down such words, certainly in a professional journal. It is noted here only as a perhaps partial explanation of the writer's viewpoint and judgments.

I would not trouble to note his odd reference to myself, except that, in a review of a work engaging close to forty contributors and covering our entire succession of justices, high disinterestedness and care is in order. Mr. Graham's magisterial judgments might be pardoned as evocative of others, but his specific statements of alleged fact raise important questions of responsibility.

Mr. Graham presumes to comment on my "overcommitment" as having prevented me from capitalizing on works he vaguely describes as "regional and subject." He imagines that a more "adequate coverage" was in order, not only with respect to Harlan, Gray, and Waite, but also Woods and Matthews, "on all of whom important work has recently been done . . . some not cited at all." I will only in passing refer to my long, satisfying, and amply requited labors in these themes and for these volumes. But what does the reviewer presume to mean by "overcommitted"? And what is this important and, one must assume, mind-changing work that has been done, particularly in respect to Woods and Matthews? Many persons have opinions about Harlan and Waite, and they may scan my essays for themselves, and will, to determine what mysterious ingredients have been withheld.

But let them also judge for themselves whether the essays on Matthews and Woods and even Gray have not added to their knowledge and understanding, especially in synthesis with historical and related data. Let them also judge whether the essays have or have not digested other materials in a normal research manner for the use of readers general and professional. Take Woods, whom all the authorities name-dropped by the reviewer have set down in passing as a mere "Southern justice." How many had occasion to so much as recall that he was an Ohioan who did much to bring that state into defense of the Union and who served it as a general in the field? And that sev-

eral of his important decisions did not at all necessarily serve "Southern" but national purposes?

The reviewer's obscure reference to "overcommitment" is impudent and absolutely uninformed. But if he is not to be convicted of rant and vanity, he must respond to the above, and with something that is not merely "Graham, *Everyman's Constitution* (1968)."

LOUIS FILLER

Antioch College

MR. GRAHAM REPLIES:

Professor Filler and I are fellow disappointees. Few constitutional problems have excited more interest, controversy, and research than has the corporate "person." Together with Roscoe Conkling's *Argument* and forgeries, Chief Justice Waite's enigmatic *Santa Clara* "dictum" (actually a fluke of court reporting, we learn from the Waite Papers) served as a take-off point for judicial-historical interpretations and misinterpretations that culminated in the rise and disproof of the conspiracy theory, in eventual recovery of the antislavery origins of the Fourteenth Amendment, and much else. Such *causes célèbres* are generally among the public utilities of history; and here, where things rarely were what they seemed, the denouements themselves have been pivots and revelations. The final denouements came in 1963 (see Peter Magrath, *Morrison R. Waite* [1963], 221 ff): court reporter Davis' inquiry about the form of the memorandum in the *Santa Clara* case and Waite's "I leave it with you" reply—that fluke that not only climaxed Justice Field's persistent maneuverings while on circuit but that sank, without a trace, Justice Woods' express holding of 1871 that corporations were not "persons" (*Everyman's Constitution*, chs. 9, 13, *passim*).

The startling thing is that not a word of these recent discoveries and denouements got assimilated into Filler's essays on Waite or on Woods (*Justices of the United States Supreme Court*, 2: 1253, 1327). And not even a reference in the essay on Stanley Matthews to Magrath's chapter, "Court Packing in the Age of Enterprise"—one of the best accounts of Matthews' railroad associations and nomination to the Court.

Dismayed at what "haste and spurs," publishings' "compulsions and deadlines," seemingly had cost in these instances and others, I expressed, and re-express, surprise and disappointment: "It is unfortunate, for example, that overcommitment should have prevented Louis Filler from capitalizing on [his known] regional and subject interests and from providing a more adequate coverage of Harlan 1 and Gray, and of the three Ohio justices (Waite, Woods, and Matthews), on all of whom important work has recently been done, some cited here but unassimilated, some not cited at all."

Wherever it is apt and significant, reviewers quite properly evaluate contributions in part by the use made or not made of their own work. That Professor Filler's comparatively brief, readable, but in my judgment thin, unfocused, and dated *Lives* were satisfying to prepare is not at issue. In question are only the "adequacy" of coverage, insight, and synthesis; how much and how well the *Lives* do assimilate, consolidate, and clarify, using available materials, cited and uncited. Of these matters, as of my "care" and "responsibility" as reviewer, readers and students of the Court and period must and will judge.

HOWARD JAY GRAHAM
Los Angeles, California

TO THE EDITOR:

In a review of a book entitled *Studies in Philippine Church History* (*AHR*, 76 [1971]: 180) the reviewer, M. P. Onorato, makes the following statement: "The nationalist Philippine Independent Church might have gained the loyalty of most Christians if Governor William H. Taft had not favored the Roman Catholic Church." Apparently this sentence and what follows by way of proof represent the opinion of the reviewer, and since the sentence quoted is a travesty of a very important aspect of American policy in the early years of American occupation I feel it necessary to give as brief an account as I can of that policy as I remember it. No doubt it has been treated in many standard works, but I have not attempted to make a comparison of them, hoping that what I have to say has some value as an eye-witness account, being founded either on events that I remem-

ber very well or on my father's letters and conversations.

My father went to the Philippines in the spring of 1900 as the head of a commission appointed by President McKinley to set up a civil government. He became the civil governor on July 4, 1901, succeeding General Arthur MacArthur, who had been the military governor, and returned to the United States during the following winter to undergo a serious operation. As soon as he had recovered he went to Washington to report to President Theodore Roosevelt and to Elihu Root, who as secretary of war was responsible for our policy in the Philippines. One of the most pressing concerns of my father and his colleagues in Manila, which had been repeatedly stated already in personal letters and dispatches, was the purchase by the Philippine government of the "Friars' Lands," some of the best agricultural land in Luzon and other islands, which the Spanish king had bestowed on the Dominican and Franciscan orders in the seventeenth and eighteenth centuries. This transfer of real estate would not only have the effect of returning the full possession of good farm land to the Filipinos but would also rid the islands of communities who had lost whatever spiritual influence they might once have had and who were universally hated by the Filipino people. The president and Mr. Root were already converted to taking such action, and Archbishop Ireland, adviser to McKinley and Roosevelt in such delicate matters, was summoned from St. Paul to be consulted as to the best manner of approaching the Vatican, with whom the agreement would have to be made. Roosevelt insisted that my father must go to Rome as his representative to negotiate the deal. My father was a Unitarian and before he went to Manila had no experience dealing with the Catholic hierarchy. It was decided to send with him Bishop O'Gorman of Sioux Falls, South Dakota, who had visited Rome frequently and was thoroughly familiar with the inner workings of the papal court.

In the spring of 1902 we were all in Rome and were most kindly received by Pope Leo XIII. My mother, my brother Bob, and I were given a separate audience, and although the pope was over ninety I remember him as a most vigorous and charming man. My father's nego-

tiations were chiefly carried on with Cardinal Rampolla, the papal secretary of state, who was also very friendly and reasonable. It was a disappointment when my father received word that the Vatican could not agree to the sale but would send a papal legate to Manila to investigate the situation. My father thought, probably as the result of inside information received, that the Dominicans and Franciscans in Rome had been responsible for blocking papal action.

He returned to Manila in May 1902 in the midst of a cholera epidemic and found that a Filipino priest named Aglipay was making enormous strides in founding the Filipino National Church. My father objected to their seizing the property of the Roman Catholic Church, including the churches, but otherwise had no fault to find with Aglipay and his followers. He always spoke of Aglipay as the "Filipino Pope," but I don't know what title he really assumed. In September the papal legate Archbishop Guidi arrived, and again my father found him a most friendly and affable person with whom to carry on negotiations. I can here contribute an incident that is based on a very clear memory of the scene and on my father's account immediately afterward of the conversation. We were living in Malacañan Palace, a rambling, spacious building in which the reception rooms were all on the second floor, approached by a broad and imposing staircase from the carriage entrance. My mother regularly held public receptions that were attended by the American colony and the elite of Filipino society. Soon after he reached Manila Archbishop Guidi attended such a reception wearing his full regalia and arriving early. After chatting with my mother and father he retired to another room and formed a circle of his own. Word reached him that Aglipay, also in full regalia, was ascending the stairs. He went hastily back to consult my father, who attempted to explain that under the American Constitution Aglipay had as good a right to be there as anyone else. Guidi was appalled and was emphatic that he could not meet him. My father therefore arranged for the papal legate to make his exit by another route and then received Aglipay with due courtesy. Perhaps the situation was eased by the fact that Aglipay could speak no English and my father no Spanish and all explanations could be avoided.

Incidents of this kind may have helped to convince the papal legate that no time should be lost in coming to terms with the American government if the Roman Church were to keep its hold over the Filipino people. Pope Pius X embarked very quickly on a policy of substituting American and Filipino bishops for the Spanish hierarchy, and the dark shadow of Spanish ecclesiastical rule that had been a handicap in the development of the islands for three centuries began to recede. Nevertheless the deal on the Friars' Lands was not completed until November 1903, after a year of haggling over the price. It was supported by an act of Congress that authorized a bond issue by the Philippine government to cover the eight million dollars needed. The land was then sold in small farms at a moderate price, and fifty thousand Filipinos were said to have become landholders or increased their holdings. This business deal, which was the Church matter to which my father gave the most attention, was completed only a few months before he left Manila to take office in Theodore Roosevelt's cabinet, and as far as I know neither he nor his successors took any further part in Church affairs. If they had (quite inconceivably) given enthusiastic support to the Filipino National Church their memory might be more cherished by some Filipinos today, but the transfer of Church lands could scarcely have taken place without a revolution.

HELEN TAFT MANNING
Bryn Mawr College

PROFESSOR ONORATO REPLIES:

In her letter Professor Manning asserts that my statement, "The nationalist Philippine Independent Church might have gained the loyalty of most Christians if Governor William H. Taft had not favored the Roman Catholic Church," is "a travesty of a very important aspect of American policy in the early years of American occupation." I wish that Mrs. Manning had provided me with more than just anecdotes to rebut. Had she read the pertinent article "Iglesia Filipina Independiente" in the work I reviewed, Mrs. Manning would have learned that her father disapproved of Aglipay's efforts to secure the favor of the American imperial authorities and their Filipino collaborators, that her father disliked Aglipay and said that "the establishment

of the church under priests of the character of Aglipay and his fellow schismatics will not be to the interest and good morals and religious betterment of the Islands" (p. 238 n.45), and that her father's decision concerning those Church lands fought over by the Catholics and Aglipayans helped the Roman Catholic position in the Philippines. It goes without saying that Governor Taft did not use his high office to further the Catholic Church. He was hardly neutral, however, in the contest between the two factions; he could not afford that luxury. The Aglipayan Church represented nationalism and stood for Filipino independence. Taft abhorred both movements. The Catholic Church, on the other hand, scrupulously avoided the issues of nationalism and independence. In 1916, for example, Cardinal Gibbons of Baltimore, among other Americans, lent his name to the effort to defeat the Clarke Amendment (providing for Philippine independence by 1920) to the Philippine Organic Act, or Jones Act. In the final count many Irish Catholic representatives joined Republicans to defeat the amendment. Professor Taft of Yale was very pleased by the cardinal's good sense. As for the Friars' Lands, Professor Manning ought to know that their purchase by the United States government worked to the advantage of the Catholic Church in the Philippines, which was relieved of a burden at a time when ownership of agricultural lands by the Friars was impolitic. Governor Taft, naturally, sought only to establish harmony and remove a dangerous issue. He was not concerned with the Catholic Church *per se*. The manner in which he pursued the controversy, however—an audience with the pope as well as an attempt to extend the hospitality of Malacañan Palace to the papal legate—must have given the Filipino people the distinct impression that the Catholic Church was still so powerful in the Philippines that the Americans had to handle it with kid gloves. No one courted the Aglipayan Church!

As for the other American imperial administrators, it is true that, as Mrs. Manning contends, they did not take any active part in Church matters after the purchase of the Friars' Lands. But the fact is that they were very careful not to offend the Catholic Church on such matters as educational policy, the employment of public school teachers, the materials used in textbooks, and divorce and the family. They were equally

concerned that the efficacious work of the American Protestant churches among the Filipinos, especially the non-Christian tribes and Muslims of Mindanao, go forward. But, in the last analysis, they were cognizant of the powerful lobby that the Philippine Catholic Church had in the American Catholic hierarchy. A realistic appraisal of the situation demanded that they offend the Catholics the least.

Governor Taft and his successors did not favor the Catholic Church because of any desire to further its ends. But through their actions they tipped the balance against the Aglipayan movement.

Finally, since I believe it to be important, I must say that I am not Filipino, hold no brief for the Aglipayan Church, and none against William Howard Taft. The only brief I have is that of never committing a travesty upon the fair name of Clio.

MICHAEL PAUL ONORATO

California State College, Fullerton

TO THE EDITOR:

This is to express my pleasure at Russell E. Durning's sympathetic and perceptive review of my book, *The Roman Years of Margaret Fuller* (*AHR*, 75 [1969-70]: 2015-16). I am afraid, however, that I am guilty of failing to stress one important point. Contrary to Professor Durning's impression, I did in fact have something new to report on the marriage of the Marchese Ossoli and Margaret Fuller—though I so buried the source in a footnote that it has been frequently overlooked.

I published, for the first time in English, an extract from a letter from Ossoli's sister (pp. 291-92) that stated that Margaret and Ossoli were married after they came to Florence—that is, more than a year after the birth of the baby. This is the only precise evidence about the marriage thus far uncovered by anyone. Apparently the letter had escaped destruction by the Fuller family because it was written in Italian and was lengthy and difficult to translate.

Also worth noting is the fact that the police document in which Margaret gave her place of birth as Rome, her maiden name as Ossoli, and her age as twenty-nine (she was thirty-nine) has never before been translated or published; the same is true of Ossoli's *procura* concerning the

baptism of the baby. Nor were more than two or three of the Italian love letters published.

Professor Durning is correct in saying that the biography is "the most complete record yet published" of those dramatic years in Margaret Fuller's life. The credit due her as an outstanding "liberated woman" is very much on the agenda.

JOSEPH JAY DEISS
Wellfleet, Massachusetts

PROFESSOR DURNING REPLIES:

I appreciate the gracious tone in which Joseph Jay Deiss amends my review in the above letter. In self-defense I can say only that I overlooked this information about the marriage when I wrote my review, even though I see now that I underlined it in my review copy. The letter from Giovanni Ossoli's sister is indeed important new information about the marriage, and I can only wish that Mr. Deiss had given it greater emphasis in his book with more of the background surrounding the question of the formal marriage ceremony.

RUSSELL E. DURNING
Northern Illinois University

TO THE EDITOR:

In his review of the correspondence between Marie von Ebner-Eschenbach and Dr. Josef Breuer, 1889-1916, which I edited (*AHR*, 76 [1971]: 495), Rudolph Binion writes that "the historical interest of this correspondence is ill served by the present edition. Some surviving letters were omitted, others elided." He has, of course, not seen the material whose deletion he deplores; however, he fails to refer to what he has seen, namely my clear explanation in the preface (pp. 13-14) of why some—usually brief—letters were not included and certain passages in others omitted. Quite apart from the question of general interest, a patient, even many decades after his death, has the right to privacy in regard to the ailments of old age. In other instances minor exchanges of news about the large families of both correspondents were eliminated not to burden the letters with what many readers would consider trivial. All these omissions were, however, held to a minimum.

The fact that the correspondence began in 1889 although the relationship had commenced

several years earlier is hardly surprising. Breuer and Ebner-Eschenbach lived in the same city, and the correspondence became voluminous only when the latter began to spend most of her time either on the family estate or in Rome. Two letters out of nearly two hundred were printed slightly out of chronological order. They came into my possession much later than the bulk of the manuscript and were correctly inserted; in the long, drawn-out process of publication they were subsequently misplaced. *Habent sua fata libelli!* Professor Binion further criticizes the fact that the notes are too sparse and the chronological charts too scanty and that an index is lacking. I did what I could within the limited space allotted to me, but the reviewer is in a sense correct. This small volume is one in a series of very low-priced books on outstanding Austrians, published with a minimum of apparatus. Although they are of decided cultural merit, none of the volumes I have seen has an index or notes, and chronological charts and bibliographies are even more limited than are those in my volume.

As to the "comically scant biographic information" provided in the biographical charts, Breuer's life was devoid of dramatic external events. Yet his three basic scientific discoveries are listed. Regarding Ebner, the facts necessary for the understanding of the correspondence are given. The same principle pertains to the "redundancy" of references to family matters in the notes. As to the selection of the illustrations I was bound by a kind of family parity.

I had the choice of waiting an indefinite number of years, if not forever, before a more elaborate edition of the correspondence could be published or of bringing it in its present form before wider strata of the Austrian public who know much of Ebner-Eschenbach and practically nothing of Breuer. I chose the latter course and do not regret it at all. The content of the book is of high literary significance and great scholarly interest, whether the frame is the ideal one or not. On the other hand Breuer's correspondence with a writer who is considered an Austrian classic offered the best opportunity to acquaint the Austrian public with the personality of a man whom Professor Binion rightly characterizes as "a savant, superlatively learned, clever and humane." I do hope that the knowledge of Breuer's personality obtained through

this publication and the numerous favorable reviews published in German will make it possible to edit some of his weighty philosophical and psychological writings.

While I apparently rate Marie von Ebner-Eschenbach's personality and her admirable endeavors to learn and comprehend a world she never made much higher than does the reviewer, I am particularly puzzled by Professor Binion's last paragraph: "The introduction . . . somewhat introduces Breuer; Ebner was considered to need no introduction. Ebner's picture alone adorns the jacket: she is the star. Breuer steals the show." In the matter of the book jacket the reviewer is right: I was presented with a fait accompli, not of my choice. But otherwise, does he deduce from the fact that I introduce Breuer at some length and not Ebner that she is "the star"? Again, as clearly explained in the preface, Ebner is not introduced because she is known to the general Austrian public, while Breuer unfortunately is not. The whole terminology is, of course, inappropriate. Ebner, whom I greatly respect, was not a star, and Breuer, a very great man indeed, did not have to steal a show. The beautiful relationship of two people, one of rarer intellect and both of extraordinary kindness and understanding, has been and will be fully understood by all those who read it in good faith.

ROBERT A. KANN
Rutgers University,
New Brunswick

PROFESSOR BINION REPLIES:

The historical interest of documents is ill served when they are incompletely disclosed. Matters of health and family occupy biographers who have passed through that "portal" that, to quote Professor Kann, Breuer "before and with Freud was the first to open" (p. 8). Researchers are the more disadvantaged when original documents that have been truncated in print are inaccessible: I hope this is not the purport of that "of course" in Professor Kann's third sentence. The issues between "general interest" and historical interest, and between a dead patient's "right to privacy" and a historian's right to pry, were not for me to air as an *AHR* reviewer, expressly concerned with the value of an edition of correspondence from the professional standpoint.

The "historical interest" of that correspondence, I wrote, is ill served by that edition, the deletions being one of seven reasons I then gave.

Another of these reasons was: "Basic contextual indications are lacking, such as when the correspondents met. We are told only that they began corresponding in 1889, a fact that is bewildering in that the first letter printed is one of 1889 in which Ebner styled herself as Breuer's old friend." I might have added that the letters are prefaced by some verse of Ebner's dated early 1883: "a painfully loud" "reproach" to her "doctor and confidant" for having neglected her otherwise than "medically" a "whole long year" (p. 17). This can only intrigue readers about the earlier relationship. The editor's historical job was to inform readers about it—at the very least to date its beginnings. Now he explains why a correspondence began only after many years of that relationship. His explanation would belong with the published letters except that it is untenable. Vienna's literati of that time, including Ebner, corresponded incessantly, even inside Vienna, and Ebner regularly divided her year between Vienna and the family estates (also St. Gilgen beginning in 1889) for decades before she moved to Italy in 1898. The published Breuer-Ebner letters are all Ebner's for the first nine years, so presumably (here again we are not told) Breuer's prior to 1898 are simply missing. Does the editor know that Ebner's prior to 1889 are not also simply missing? Now he writes that the correspondence "became voluminous" (in 1889?) as a variant of "began." This softens the one hard "fact" he had provided about its beginnings (pp. 7, 13), sharpens my point about "basic contextual indications," and opens up new vistas of uncertainty.

Nowhere did I imply that Professor Kann specifically was to blame for all the shortcomings of "the present edition." I merely reviewed, from the scholarly vantage, the results of his compromise with a publisher for whom scholarship is a drag. Granted, Breuer's vita in ten entries (with one date missing) is better than no vita for Breuer: are these the terms on which editions of letters should be judged in the *AHR*? "Ill served" denotes poor service, not no service.

I did not rate Ebner, high or low. I did intimate that she would need some introducing in a critical edition of a correspondence with her. Her name is well known to Austrians; her life

is not. My statement that "she is the star" does accord with her being, as against Breuer, a "name," but it follows from "her picture alone adorns the jacket," as is clear from the punctuation. Actually her portrait on that jacket is rather theatrical, and her name there tops Breuer's—to which "Dr." is prefixed as if to help him out. In any case, the "star" terminology does suit a presentation aligned on Ebner's renown rather than on the Breuer-Ebner relationship presented through what was emphatically a correspondence, and one in which "rarer intellect" shows on Breuer's side at that.

My review is no anthology piece. Yet it stands, except the word "fact" for that solitary, bewildering allegation about the background of the correspondence published. The historical interest of that correspondence has been ill served for the reasons I gave—and no less so, it now emerges, where Professor Kann did the serving than where his publisher did.

"Good faith," Professor Kann?

RUDOLPH BINION
Brandeis University

TO THE EDITOR:

In a recent issue Christopher Thorne analyzed "The Basis of British Policy" (his subtitle) in response to the Japanese attack on Shanghai in January 1932 ("The Shanghai Crisis of 1932: The Basis of British Policy," *AHR*, 75 [1969-70]: 1616-39). An intriguing aspect of the Foreign Office discussions about British policy, not mentioned by Thorne, was an exchange concerning the sources of Japanese conduct. More reminiscent of debate among historians than among the subjects of our research, a group of upper- and middle-level Foreign Office officials argued the relative importance of economic versus political factors as a key to understanding Tokyo's conduct in the Far Eastern crisis. Their conclusions were no more definitive than the conclusions of historians who have determinedly addressed themselves to the issue of economic or political primacy. Nevertheless, to note the inconclusive nature of the debate or to criticize its lack of rigor would be to risk overlooking its significance. Each participant revealed not only his assumptions concerning Japanese action, assumptions that then helped shape British Far Eastern policy during the thirties, but gen-

eralized about the roots of policy making by all other countries as well.

It will be recalled that the Japanese precipitated the Far Eastern crisis in September 1931. Units of the Kwangtung army occupied Mukden, Manchuria, following a fabricated Chinese attack upon a Japanese-controlled railroad and then extended operations until Chinese resistance in Manchuria had been thoroughly crushed. Along with the rest of the world, the British government was caught by surprise and, although clearly distressed, remained unwilling to threaten the Japanese with either unilateral or multilateral sanctions. In January 1932, however, the situation suddenly worsened. The Japanese navy launched a major assault upon Shanghai in order to break the back of a damaging Chinese boycott of Japanese goods begun in retaliation against the Manchurian occupation. Not only did the severity of the fighting shock the sensibilities of world leaders, but the spread of hostilities seemed seriously to jeopardize British economic interests in the Far East. Unlike distant Manchuria, Shanghai, with its International Settlement, was the center of British investment and commercial activity.¹ It was this situation that precipitated the Foreign Office debate over the roots of Japanese policy.

Deputy Undersecretary of State Sir Victor Wellesley, an official whom Thorne accurately describes as representing "the views predominant at the time" (Thorne, 1629), started the debate in a memorandum of February 6, 1932. "The root trouble in the Far Eastern problem," he wrote in an introductory paragraph, "will be found to lie (as in the case with most international problems) in economic causes. The in-

¹ British commercial interests in Shanghai alone were estimated by the Department of Overseas Trade at £63,000,000 in 1927. Total British interests in China were estimated to be between £200,000,000 and £250,000,000. It should be added that Britain shared heavily in the administration of the International Settlement in Shanghai, where British interests were localized. See Wellesley memorandum, Feb. 1, 1932, *Documents of British Foreign Policy, 1919-1939*, 2d ser., 10 (London, 1965): 288 (hereafter *DBFP*). Permanent Undersecretary of the Foreign Office Sir Robert Vansittart had written on February 1 that "if Japan continues unchecked and increasingly, as she seems bent on doing, our position and vast interests in the Far East will never recover." Minute to memorandum by Sir John Pratt, Feb. 1, 1932, *DBFP*, 9: 282.

creasing population of Japan; the increasing industrialization; the need of the China market; the increasing (Chinese) population of Manchuria; the heightened Chinese tariffs; the decrease in Japanese trade with China; the effect of the economic crisis, with the effect of the intensified Chinese boycott superimposed thereon.

"There is an increasing economic disequilibrium between Japan and China. All the present troubles are merely symptoms of the disease, and no solution of this problem is likely to be found by dealing with the symptoms only. It is the causes of the trouble which have got to be removed, and these are economic" (Wellesley memorandum, Feb. 6, 1932, *DBFP*, 9: 374-83). Following from this, Wellesley went into considerable detail on each point. His conclusions, however, were clear from the outset: the need for Japanese access to markets and raw materials "is a matter of life and death to her." Her industrialization may bring "in its train . . . the usual causes which in the past have made for war: over-population, markets and clients at all costs, and a possible revival of the imperialistic spirit." If China closed her doors to Japanese manufacturers and halted her access to raw materials, "*Japan would be forced to fight for her very existence.*" To reach a solution of the problem solely on judicial grounds would lead to "disaster." "It is for this reason," Wellesley concluded, "that I think that we should take the lead in getting the whole problem of the Far East examined by the League from the economic point of view, which offers the only hope, if any, of anything like a permanent remedy" (*DBFP*; italics in original).

Wellesley's point of view was distinctly pro-Japanese. So, too, were the sympathies of Sir Francis Lindley, the British ambassador in Tokyo. Lindley, however, rejected Wellesley's economic emphasis. In a dispatch to London in late April he called Wellesley's memorandum of February 6 "one-sided," then added that "before the war it was perhaps the fashion not to give sufficient weight to economic causes when examining political problems. It has now, perhaps, become the fashion to go to the opposite extreme; and I submit that past history and current events show that, in the last resort, political motives usually, though not of course invariably, over-ride economic considerations."

Lindley briefly turned historian to prove his point. Although he recognized that, as Wellesley claimed, Japan needed access to Chinese markets and raw materials, he argued that this condition only resulted from the exclusion of Japanese products and citizens from Australia and the United States. Lindley firmly traced such exclusion to political motives. For instance, while he admitted that Americans justified exclusion of Orientals by claiming that it preserved a higher standard of living for native workers, Lindley nevertheless dismissed the argument as "mere justification." Otherwise, why were not the Portuguese—who had an even lower wage scale than the Japanese—subject to special prejudice in the United States?

Other nations, too, provided Lindley with arguments. The ambassador believed that many nations refused to recognize the Soviet Union for political reasons (fear of subversion) in spite of their desire for Soviet markets. Ireland, in the face of economic hardship, nevertheless sought political independence from Britain, while the ancient Turkish destruction of Smyrna and its expulsion of an industrious Greek population could only be interpreted as an act of political passion in the face of contrary economic interests. Returning to contemporary events Lindley maintained that the Chinese boycott of Japanese goods must be seen as solely political, not economic (without a strong army, the Chinese used the boycott as their most effective weapon), while he asserted concomitantly that Tokyo's military leaders "are singularly indifferent to anything affecting trade and commerce." To these leaders, Lindley claimed, Manchuria represented a "political ideal." The ambassador concluded with a bit of sermonizing about British reasonableness in contrast to the political passion by which other nations were swayed; yet he felt even this might not persuade his countrymen to reject "a cession of the West Indies in return for financial assistance from the United States" (Lindley to Sir John Simon, *DBFP*, 10: 373-75).

Historians might conceivably find Lindley convincing; a group of Foreign Office officials did not. Frank Kenyon Roberts of the Far Eastern Department, in a minute attached to Lindley's letter, noted the difficulty of distinguishing economic from political motives in a state's action and admitted that political ends might, at

times, override economic considerations. "But this does not," he continued, "alter the fact that any rational state policy must in the long run be influenced and largely guided by such economic considerations as the level of population and degree of industrialization of a state" (minute, May 16, 1932, FO 371/16170/352, Foreign Office Archives, Public Record Office, London).

Douglas MacKillop, a first secretary in the Far Eastern Department, concurred with Roberts. While noting the possibility of "interminable" argument on the subject and the sense in which both economics and politics had "interlocked" in Japan, he nevertheless felt that "one ventures to think that [Lindley] under-estimates the extent to which nations generally are pre-occupied at present with the problems of their material needs and the means of supplying them." He granted that the ambassador was correct in believing that "individuals and groups have always been, are, and will continue to be moved by considerations other than those of their material welfare, which indeed are often overridden: but [Lindley] would hardly argue that, if the economic needs of Japan point in a certain direction, that direction is the *least* likely to be taken." To clinch his point MacKillop quoted in part from the original Wellesley memorandum: "'If China were to close her doors economically against Japan, Japan would have to fight for her very existence' is the theme of the memorandum to which Sir F. Lindley refers: but he would apparently say that Japan has had to fight indeed, but not for her very existence though that was at stake!" (FO 371/16170/352). The depression and the Chinese conflict had caused the dangerous economic situation in which Japan found herself; this, in MacKillop's estimation, is what Lindley seemed to ignore.

Still another Foreign Office official joined the case against Lindley, recognizing its complexity but charging that the ambassador's dispatch contained "half-truths." Assistant Undersecretary George Mounsey was somewhat more restrained—and more muddled—than his colleagues: while also admitting that political considerations have occasionally predominated at crucial moments in world history, he stated that "the question remains whether these are not mere aberrations which cannot withstand the reaction of the underlying economic causes iso-

lated thereby, whenever the time comes for these causes to reassert themselves" (FO 371/16170/352).

It seemed entirely logical during the greatest economic crisis in memory that such "causes" would eventually reassert themselves, and it therefore comes as no surprise that these very conservative British officials agreed with a generation of British Marxists that economics, not politics, was the prime mover in the affairs of nations. Their responses were not doctrinaire; they all noted the complexity of the issue and the occasional ascendancy of political forces. Yet they seemed to believe—Lindley included—that economics should, in the final analysis, predominate in the making of national policy. When Roberts wrote of "rational state policy" and Lindley of British "reasonableness and lack of political ambition," each meant that a clearheaded view of a state's economic interests must be the rock upon which a sound foreign policy would rest. Lindley, it should be emphasized, in no way disagreed; in fact, more than any of the others, he delineated these thoughts in his dispatch. The problem he saw was simply that most states did not act in this sensible fashion; passion, not reason, clouded their vision.

Unfortunately Sir John Simon, the foreign secretary, never added his own views to those of the others. but the permanent undersecretary of state, Sir Robert Vansittart, penned a minute that, more than any of the others, summed up the difficulty. Whether out of timidity or wisdom or both, Vansittart scrawled: "Novalis affirmed that to philosophise is to generalise; and the older I get the more I mistrust the ancestral pastime, whether applied to nations, sexes, 'or any other adversity,'—including this one" (FO 371/16170/352). After this it would have taken considerable nerve to continue the debate. No one tried.

GARY B. OSTROWER
Alfred University

TO THE EDITOR:

Peter Loewenberg's study of the adolescence of Heinrich Himmler ("The Unsuccessful Adolescence of Heinrich Himmler," *AHR*, 76 [1971]: 612-41) interested me not only as a historian who has dedicated two of his books to the

general analysis and history of dictatorship¹ but perhaps even more because of the fact that I happened to be Himmler's classmate, and during those school years 1910–13 even his personal friend. Dr. Loewenberg defines Himmler as "a schizoid personality who was systematic, rigid, controlled, and restricted in emotional expression in a pattern that is consistent with what psychoanalysis defines as the obsessive-compulsive character." A person of this type, according to Dr. Loewenberg and to the Freudian school to which he belongs, is an individual "whose object relations are intact and whose character has regressed to the anal mode" (pp. 616–17).

My purpose is not to dispute the theories of the Freudian school or the fact that Himmler might have been one of the types to which it has dedicated special attention. Quite the opposite: I am of the opinion—expressed in my studies—that the various neurotic types apt to play dictator are as numerous as bacteria. The task for the historian, as I stressed in the works cited and would like to stress here, is not this kind of analysis as such—which the historian is happy to leave to the psychoanalyst—but, rather, the thorough investigation of the historical conditions (not seldom class antagonisms) that made such men as Himmler wish to act and the surveying of the specific social and political conditions that enabled some of those many "bacteria" to become "socially virulent." Thus, when the psychoanalyst invades the historical field—and no one is happier than I to see him there, since I welcome the *AHR*'s attempt to support interdisciplinary research—he must pay proper attention to these tasks, lest he invite disaster. Unfortunately Dr. Loewenberg, though he practices both disciplines, has not, it appears to me, avoided these pitfalls.

It is perhaps a pity that Dr. Loewenberg did not consult me prior to publishing his piece; my connection with Himmler was stated in the bibliography of Bradley F. Smith's *Heinrich Himmler: A Nazi in the Making, 1900–1926* (Stanford, 1971),² which Loewenberg cited and

which has ample quotations from the first of my two publications on Himmler.³ As it is, Loewenberg has relied much too heavily on the Himmler diaries in the Hoover Institution, which start only with 1914 and which are, of course, one-sided and likely to cover up essential facts. Had Dr. Loewenberg checked with me, he could have verified the following essential information.

The chief characteristics of the entire Himmler family were loyalty to the royal house of Wittelsbach and devotion to the Catholic Church. Young Heinrich—who wore a pince-nez and even at that time looked very much like the later Reichsführer SS—was in the hands of his teacher of religion, a Jesuit. Heinrich's supercorrectness was inherited from his father, a high-school teacher in Munich who was known to everybody for his almost ridiculous kowtowing to royalty (he managed to persuade a Prince Heinrich, whom he tutored, to become godfather to his son). Heinrich was, in addition, the best—or one of the best—in the class and was a veritable teacher's pet.

There was, however, one endeavor in which Heinrich failed completely: gymnastics. During a class reunion in the 1960s all of us were unanimous in the opinion that Heinrich's constant exposure to public ridicule by our gymnastics teacher, Hagenmüller, a bearded Teuton who amused himself by ordering Himmler to perform the knee turn around the horizontal bars, helped to shape Himmler's psyche. His utter inability to execute such orders must have hurt him terribly. He tried in every possible way to make up for this and, for instance, taught me diving—an activity he cherished, since he intended to become, as he once con-

diaries. It appears that Dr. Loewenberg relied too heavily on this first publication and ignored the correcting statement I published in a letter to the editor, *Journal of Modern History*, 32 (1960): 212, as well as the more careful statements in Bradley Smith's second publication, cited in the text.

³ The first of these publications, prepared in the early 1950s for the information of West German readers, did not find a publisher for several years because, it was felt, my account of Himmler's early days made him look too harmless. It was finally published in the then new periodical *Germania Judaica*, 2 (1960–61), under the title "Mein Mitschüler Heinrich Himmler." More detailed are the passages on Himmler contained in my memoirs, *Als die Schatten Fielen* (Berlin, 1969), 34–39.

¹ *Why Dictators? The Causes and Forms of Tyrannical Rule Since 600 B.C.* (New York, 1954), and *Devils or Saviours? A History of Dictatorship Since 600 B.C.* (London, 1960).

² In an earlier piece (Werner T. Angress and Bradley F. Smith, "Diaries of Heinrich Himmler's Early Years," *Journal of Modern History*, 31 [1959]: 206–24) the world received a first glance at these

fessed under Haggenmüller's prodding, a naval officer (*Marineoffizier*). With the outbreak of World War I his chances to enter instead the career of an army officer—blocked in peacetime to nonaristocrats by the aristocracy's monopoly in this field—suddenly improved. In his mind, as I see it, his predilection for this type of activity was intended to prove that he possessed the prowess he appeared to lack. In the last year of the war he had indeed managed to be accepted and to serve as an officer candidate (though not yet in combat) when the German defeat, the outbreak of the revolution, and the establishment of the republic cut this career short, bringing about the decisive turn in his life.

Intimately connected with this turn was his sudden embracing of anti-Semitism—a decisive development caused by historical elements that Dr. Loewenberg totally ignores. Loewenberg's contention to the contrary (pp. 632 ff.) notwithstanding, before that time Himmler had not been anti-Jewish at all. This is evidenced by his close connection with me (I am a grandson of the New York Jewish banker Charles L. Hallgarten, though I was brought up a Lutheran, which was my mother's creed)⁴ and with Fritz Esslinger, the smallest boy in our class, who likewise had a mixed background. "Conventional anti-Semitism of his class and his culture," which Loewenberg contends inspired the young Himmler, just did not exist in his family, which was devoutly Catholic. It was found, rather, among some outspoken Protestants in our class, led by a certain von Meyer-Startshausen from Franconia, where anti-Semitism of a primitive brand had been rampant since the time of the Reformation. Himmler's sudden turn to anti-Semitism in 1918 and the following years was caused instead by the shock he received through the revolution, which was promoted by such figures as Kurt Eisner in Bavaria and Rosa Luxemburg in Berlin and which gave all persons of Jewish blood the *Gleichberechtigung* ("equal rights") they had been denied. Then and only then did Himmler suddenly

turn around and in his diary attack me, his former school friend, for my defense of the republic, using arguments and slogans that in former years would have been flatly unknown to him. (I had lost sight of him in 1913, when the Himmlers moved to Landshut).

Himmler's despair about the sudden collapse of the German army, where he had found a haven, is related to Hitler's experiences during those months. The unexpected turn made both men more outspoken in their feelings than most others, since both lost much more than almost everybody else. It was the German collapse of 1918 that shaped their future fate, though Himmler, according to his upbringing and general outlook, was much more of a would-be aristocrat than was the plebeian Hitler.

I would not deny that Himmler's psychology corresponded to the schizoid type Dr. Loewenberg analyzes. Loewenberg's medical presentation does explain the apparent lack of any human feeling that my once tender and sensitive former friend was to display toward the suffering of the millions of humans he was to torture and kill. But in order to activate his potential the historical experiences I described were required, just as he could never have found an outlet for his feelings had it not been for the lamentable historical evolution of Europe during the Great Depression.

Even when about to reach his goal, however, Himmler was prompted by the experiences of his youth. According to one of his biographers, W. Frischauer, when Himmler was chief of the Gestapo he erected near his headquarters at Prinz Albrechtstrasse in Berlin a gymlike sports arena for himself in which none of the torture instruments with which Haggenmüller had threatened him were missing. Preaching to everyone about the necessity of gymnastics for the German race, he ordered all his SS men to acquire the *Deutsches Sportabzeichen* ("sports medal"). The only man who was unable to acquire it was Himmler himself. The still clumsy and a bit too corpulent body simply did not obey his wishes. Then the SS examiners—according to the surviving witnesses Frischauer interviewed—used a trick. Without Himmler's knowledge they gradually changed the measurements and timers to make them compatible with his abilities. According to Frischauer—

⁴ Himmler often came to see me in our house in Herzogspark in Munich, which was then a meeting place for men like Thomas Mann, Bruno Walter, and the composer Hans Pfitzner. An invitation I extended to Himmler to visit me can still be found in Himmler's files.

who, by the way, had no knowledge of my own observations—Himmler had never looked happier than when, due to this ruse, he finally passed the test, thus vindicating the failure of the schoolboy.

GEORGE W. F. HALLGARTEN
University of Dayton

PROFESSOR LOEWENBERG REPLIES:

I am impressed by the extent to which Dr. G. W. F. Hallgarten's interpretations and observations on Himmler's personality essentially square with my own, that is, loyalty, super-correctness, and a compensatory desire for the appearance of great strength to cover weakness. There remain two substantive issues that Dr. Hallgarten has raised: the extent and nature of Himmler's pre-1918 anti-Semitism and what caused Himmler to become a Nazi and a virulent anti-Semite.

Dr. Hallgarten contends that Himmler "had not been anti-Jewish at all. This is evidenced by his close connection with me. . . ." The questionable value of citing friendship with Jews or a Jew as evidence of a lack of anti-Semitism should be obvious. Even if Himmler had said that some of his best friends were Jewish, I should not consider this acceptable historical evidence of his feelings. Dr. Hallgarten's letter also presents the issue of whether personal recollections after sixty years are more reliable than original documents of the period. The very points of inaccuracy on which Bradley Smith cites and corrects Dr. Hallgarten go to prove that memories, no matter how clear, are undependable after six decades. (See Bradley F. Smith, *Heinrich Himmler: A Nazi in the Making, 1900-1926* [Stanford, 1971], 29, 182 n.12, 183 n.34.) When authenticated contemporaneous manuscripts are in direct conflict with personal recollections, the documents must prevail unless they are obviated or placed in a new perspective by other evidence.

Dr. Hallgarten suggests that it was the shock of a revolution led by Jewish leftists such as Kurt Eisner and Rosa Luxemburg that turned Himmler against Hallgarten personally and against others whom Himmler regarded as Jewish. It is a logical and an empirical mistake to attribute anti-Semitism to the specific acts of individual Jews. As Dr. Hallgarten must

know, most German Jews were not revolutionaries but, rather, were allied with the forces of law and order; furthermore, Eisner and Luxemburg were murdered soon after the First World War. The interesting question would be why Himmler fixed on these particular personalities to react to in his ideology, if this is in fact what he did.

All historians of nazism should welcome Dr. Hallgarten's most interesting personal reminiscences. When we are in a data-gathering process any information, especially first-person testimony and oral tradition, is potentially valuable. In the case under discussion I believe that Himmler's adolescent diaries clearly indicate that National Socialism solved his personal, vocational, psychosexual, and ethnonational identity problems.

PETER LOEWENBERG
*University of California
Los Angeles*

The following letter is in reply to a communication from Carlos Martínez de Campos (AHR, 76 [1971]: 582-83), concerning Professor Carter's review of Sr. Martínez' España Bélica (AHR, 75 [1969-70]: 141-42).

PROFESSOR CARTER REPLIES:

Nowhere in my review of Sr. Martínez de Campos' *España Bélica* do I comment "that the work is not sufficiently informative," but instead—variously stated and illustrated—that it is not sufficiently well informed (or accurate or properly founded) to be taken seriously as a historical work. Nor did I (nor do I) object to its survey nature or find its length inadequate to that purpose. Even my criticism of the underlying investigation is not so much quantitative as qualitative.

Re the author's four specific complaints: (1) It is true that far more space was devoted to the conquest of Italy than to that of the Canaries, and appropriately so, but my criticism was explicitly of military bibliography, where the balance is, inappropriately, the other way round. (2) The books on the Thirty Years' War he refers to are perfectly "adequate sources," some even indispensable, but the distinction between works on individual aspects of a war and a history of that war apparently eludes him. (3) I

said "his best authority for Elizabethan England is Churchill," not that he relied solely on him; perception of meaning no more approximate than this ("best" read as "only") hardly bodes well for the scholarly product. But let us accept that misreading and add Lingard (an excellent foundation for 140-odd years of subsequent work) and the cited "Spanish works on the period" (none of which are authoritative on the subject): the author seems to be trying to make my point for me. (4) Both before and after the count of Lerma was elevated to duke of Lerma his other simultaneously held titles included that of marquis of Denia, which the author gives correctly here but incorrectly in the book as duke of Denia; if a comment about miscalling a marquis a duke is "not clear at all," I know not how to explain it further. In any case the author mentions only this least of a random sample of factual errors.

Sr. Martínez concludes by saying that my criticisms "could have been more accurate and dispassionate" and comprise an "unfair critique" whose publication in the *AHR* is "regrettable." As noted above, none of the criticisms complained of are inaccurate; the author simply fails to get the point of any of them, which is the fundamental problem involved. Though I considerably moderated earlier drafts of the review before submission, sacrificing precision of feeling in the interest of civility, I see no merit in being "dispassionate" about travesties of historical scholarship; honoring such diminishes the proud history of a great nation. My review complains that the author's privileged exemption from serious criticism is unfair both to Spanish historians and to Spanish history, while he complains that my critique is itself unfair; perhaps the matter should be judged a standoff and allowed to rest at that. I must, however, add that, as any reviewer should, I read every line of the work reviewed; no specialist who undertakes that unrewarding task will accuse me of having taken the responsibility of criticism lightly. (In fairness I must also clarify the nature and degree of my general complaint about the author's privileged status—duke, general, preceptor to the heir-designate to the Spanish throne, etc. When the review appeared more than one American historian asked whether in the course of my own researches in Spain I was not in danger of retaliation. The answer is

no. I would in no way imply that such melodramatic circumstances exist for historians there, foreign or domestic; my criticism of the situation is precisely what I said in the review and should not be exaggerated to fit exaggerated notions of that situation.) Finally, I am not surprised that any author would regret publication of so totally negative a review, but the numerous historians of Spain who have written me or otherwise commented upon it—unanimous in approving its having said something that needed saying—definitely do not. I naturally regret, however, the pain that such a review, though justified, inevitably causes the author. For both scholarly and humane reasons I prefer to review works deserving of praise.

Two errors that crept into the review (uncaught because of an accident of timing in the shuttling of edited manuscripts) should be noted: Sr. Martínez' volume on nineteenth-century Spanish military history is incorrectly referred to as a multi-author work, and in a reference to his "depending only on Spanish battle accounts until the late seventeenth century" the word "only" dropped out, leaving an unintended implication about the quality of those excellent sources. Similarly, in my adjoining review of an edition of Francisco de Vitoria's *Relectio de Indis: O libertad de los Indios* (*AHR*, 75 [1969-70]: 142-43) the comment on the unequal usefulness of the introductory essays refers not to quality of scholarship (as seems implied) but the adequacy of length to the authors' subject matter: P. Teófilo Urdániz' one hundred pages versus Vicente Beltrán Heredia's seventeen and Reginaldo di Agostino Iannarone's eleven. The lengths unfortunately dropped out, making apologies in order to the latter two fine scholars for obscuring the merely technical nature of the criticism.

CHARLES H. CARTER
Tulane University

R. John Rath regrets that in his review of Angelo Filipuzzi's *Le relazioni diplomatiche fra l'Austria e il Granducato di Toscana* (*AHR*, 76 [1971]: 494) he referred to Filipuzzi as "one of Austria's foremost experts on Italian history," when in fact he is an Italian historian who has done valuable work in both Italian and Austrian history.

Association Notes

Since the death of Professor David M. Potter on February 18, 1971 (see pp. 1273-75 of this issue), Professor Joseph R. Strayer of Princeton University has been president of the American Historical Association.

The annual meeting of the Association, which is being held on December 28-30, 1971, at the New York Hilton, New York City, is under the program chairmanship of Professor Vartan Gregorian of the University of Texas, Austin; Professor Ari Hoogenboom of Brooklyn College is chairman of the local arrangements committee.

Mrs. Phyllis M. Brown has been appointed membership secretary of the American Historical Association. Mrs. Brown has worked with the Association's membership staff for almost four years.

Recent Deaths

DAVID MORRIS POTTER, president of the American Historical Association and Coe Professor of History at Stanford University, died of cancer on February 18, 1971, at the age of sixty-one. At the time of his death he was also president of the Organization of American Historians. On his desk was the nearly completed manuscript of his study of the coming of the Civil War, which was intended for publication in the New American Nation series and on which he had been working for over a decade.

Although death cut down David Potter at the height of his career and powers, but before the publication of what would have been his masterwork, his life and work had already had widespread impact. For Potter's influence in the profession derived from more than his scholarly work, important as it is; it flowed also from the man himself. Few people—students or professional colleagues—who came into contact with him remained unaffected by the experience.

David Potter was born on December 6, 1910, in Augusta, Georgia; he attended schools there until 1928, when he left for Emory University. At Emory he was a debater, a member of Phi Beta Kappa, and a companion of C. Vann Woodward. After obtaining the B.A. from Emory in 1932 Potter began his graduate studies at Yale under another Georgian, U. B. Phillips; Yale awarded Potter the Ph.D. in 1940. Meanwhile he had begun his teaching career in his native South, spending two years at the University of Mississippi and four years at Rice Institute. When his dissertation appeared in print in 1942 as *Lincoln and His Party in the Secession Crisis*, he returned to teach at Yale, where he remained for nineteen years. It was during his long tenure at Yale that he revealed

his diverse talents and high sense of duty to the institutions he served. At different times, for example, he was editor of the *Yale Review* and director of the program of American studies at Yale. During those same Yale years Potter went abroad to be Harmsworth Professor at Oxford and to Chicago to deliver the Walgreen Lectures, which were later published as his best-known book, *People of Plenty* (1954).

In 1961 Potter moved to Stanford University, where he was Coe Professor of American History and, for three years, executive head of the history department. During his years at Stanford Potter also journeyed to lecture posts in England and the United States. He gave the Commonwealth Fund Lectures at the University of London in 1963 under the general title "The Compulsions of a Voluntaristic Society: Individual Freedom and Its Limitations in American Life." In 1968 he delivered at Louisiana State University the Walter Lynwood Fleming Lectures under the title "A Century of the Concurrent Majority." At Stanford, as at Yale, Potter contributed his time and talents to the work of the university and the department, serving on time-consuming appointive committees and elective faculty bodies, some of which were important and others of which were not. But Potter was never one to stand on his prestige; he served when and where he was needed.

All who heard him speak or knew him recognized his love of paradox. The hundreds of students who each year elected his courses commented frequently on his ability to discover paradox and irony in history. This penchant for paradox was actually a manifestation of a broader faculty. To David Potter's eye things were rarely what they seemed to others; his abil-

ity to look afresh at the familiar was the key to the originality and penetration of his intellect. He often found paradox because he looked more deeply. In his very first book, *Lincoln and His Party in the Secession Crisis*, Potter already displayed this faculty, for he saw in Lincoln's problem at Fort Sumter a complexity of motivation that previous writers had overlooked but that no subsequent writer could ignore. Not surprisingly, the book brought Potter an almost immediate international reputation as a Lincoln and Civil War scholar. In the book that established his reputation as a brilliant interpreter of American history as a whole, *People of Plenty*, Potter showed how Turner's frontier thesis was a special case in a general theory of abundance. After the publication of this book the academic study of national character achieved a new respectability. Potter's most recent book, *The South and the Sectional Crisis* (1968), a collection of his essays, demonstrates in several places his ability to see the old and the familiar in a startlingly fresh way. This is especially notable in the essays "The Historian's Use of Nationalism and Vice Versa," "The Enigma of the South," and "John Brown and the Paradox of Leadership among American Negroes." Other examples of his faculty for penetrating to new insights, whether through paradox or otherwise, are spread through his dozens of essays and book reviews published over the years. Each one of these essays was composed with the same attention to the nuances of meaning and clarity of thought that he insisted upon for his books. The essays also provide a measure of the breadth of his intellectual and scholarly interests, for they range over Western history, the history of women, Southern freight rates, advertising, the New Deal, and the intricacies of historical method.

Paradox is more than a mode of Potter's thought; it also describes the man. It was always something of a surprise to him, for example, that he was a popular lecturer, for he did not possess the usual attributes of a successful public speaker. His voice was too soft; he spoke too slowly and reflectively; he "covered" little ground. Yet the iron logic of his thought, the originality of his ideas, and the precision of his diction could hold a hall of three hundred students in unwonted quiet. In 1968 he was

awarded Stanford's highest award for distinguished teaching. As a teacher he was paradoxical in another way. Undergraduate and graduate students alike could approach him without ceremony or fear, for he was democratic in manner and uncommonly generous with his time. Yet he remained as private a person as one would ever meet; his reserve could be impenetrable if he wished to make it so.

His professional life, too, had its paradoxical aspects. He was a professional historian in the fullest and best sense of the phrase. He loved teaching and writing history; he reveled in the conventions, in the shop talk, in the gossip; he willingly served on the boards of editors of the *Pacific Historical Review*, the *Mississippi Valley Historical Review*, and the *Journal of Southern History* and on the governing bodies of the Southern Historical Association, the American Historical Association, and the Organization of American Historians, among others. Yet he never confused a commitment to truth or to decency toward another human being with a commitment to the profession. As few men can do, David Potter could see himself, his work, and his colleagues in historical perspective; that is, he was rarely impressed, though always appreciative.

Paradox also describes David Potter's relations with those with whom he disagreed. Ideologically and politically Potter was conservative. (In the last year of his life he was the commencement speaker at an ROTC graduation at Stanford.) As a confirmed opponent of optimism he did not expect much improvement from any change, though he recognized that change was inevitable. Yet despite his unconcealed conservatism he enjoyed probably as many admirers of his work, thought, and personality among those of liberal and radical persuasions as among those closer to him in ideology. His intellectual integrity, his clarity of thought, and his deep-seated wish to understand were apparent to those who stood to the left as well as the right of him. Moreover, his ultimate commitment to truth—to understanding—meant that his conservatism was neither rigid nor unchanging. The operative word here, to use one of his favorite expressions, was "understanding."

Finally, paradox is evident in his judgment of others. Although a man of great intellectual

penetration himself, he was always more interested in a person's character than in his intellect, though he appreciated both. In David Potter's calculus of personal value brilliance of intellect could not compensate for overweening arrogance or deliberate unkindness.

Yet paradox, encompassing as it is, cannot capture the whole of Potter's mind and character. For it is not paradoxical, though it is true, that he was a dedicated bird-watcher and ornithologist whose bookshelves bore the weight of dozens of heavy tomes on birds of the world and whose leisure time could be spent behind binoculars scanning the Sierra skies for rare California condors. Nor is it paradoxical, though it is true, that this man of uncommon mind and personal dignity was a natural wit from whom hilarious stories, clever turns of phrase, and piquant historical facts came in an unfailing stream. Nor is it paradoxical, though it is true, that his judicious advice and reliable wisdom on personal and educational questions were sought by students, colleagues, and university presidents alike. As one of his colleagues succinctly put it, "No discussion seemed complete if he was present and had remained silent."

It may seem paradoxical, especially in an obituary in a professional journal, but it is true, nonetheless, that the loss of the distinguished historian should seem smaller than the loss of the man. To say this is not to diminish Potter's acknowledged stature as a historian but rather to record the remarkable fact that his character matched his eminence as a historian.

In gratitude and remembrance, graduate students at Stanford University have set up the David M. Potter Memorial Fund for Graduate Awards. Contributions to the fund may be addressed to: Potter Memorial Fund, 301 Encina Hall, Stanford, California 94305.

CARL N. DEGLER
Stanford University

FREELAND K. ABBOTT, professor and chairman of the history department at Tufts University, died suddenly on February 23, 1971, at Pusan, Korea, while on sabbatic leave. He held all three degrees from Tufts and since 1950 specialized in the Islamic tradition in modern South Asian history. His main contribution to the field may be found in *Islam and Pakistan*

(1968), a study of the social, political, and ideological consequences of Islam in Pakistan's quest for a national identity.

Dr. Abbott went to Pakistan first in 1953 as a Ford Foundation fellow while an assistant professor of history at Tufts. He underwent training in both the Muslim and the Hindu aspects of modern South Asian history, and his research interests were wide-ranging. Movements of intellectual change especially interested him. His early work concerned Shah Waliullah and the decline of the Mughal Empire and Sayyid Ahmad Shahid and the jihad movement.

He returned to Pakistan in 1959 as a Fulbright research professor and began the research that would lead to his major study of Islam in contemporary Pakistan. One of his first publications in this field was in Urdu in 1960, and by the end of the decade he had written on a number of issues produced by the clash between political life in Pakistan and Islam's injunctions. These works included considerations of the state's declaratory ideology, changes in the marriage law, the idea of a secular state, and religious developments in the period from 1958 to 1969 in Pakistan. At the time of his death he was returning to his interests in early modern intellectual history in the subcontinent, and *Contributions to Asian Studies* carried his last article, "The Historical Background of Islamic India and Pakistan," in its June 1971 issue.

If there was a master theme to his interests it was the interplay between Pakistan's social history and its religious heritage. As he wrote in "Pakistan and the Secular State": "It is true that Pakistan is not really a secular state, but neither is it truly a religious state; its division between two cultures cuts deep, and its role may well be to evolve a conformity, or at least a harmonization, one with the other" (in D. E. Smith, ed., *South Asian Politics and Religion* [Princeton, 1966], 370).

Freeland Abbott stood with Charles Adams, Fazlur Rahman, Wilfred Cantwell Smith, and Aziz Ahmed as one of our few interpreters of the social ethic of modern Islam in South Asia, and especially in Pakistan. The demands on the scholar in this field are many: familiarity with both the Muslim and the Indic traditions, linguistic competence in Arabic and Urdu as well as Persian, and, most important, an ability to

see the influence of ideas and entire systems of value at work on the human condition in a complex foreign culture. After almost twenty years of research and reflection Freeland Abbott had begun to meet his own high standards of care, knowledge, and intellectual synthesis, and his untimely death cuts short his creative contribution to an important and largely ignored field. For his considerable achievements represented in his book and articles all students of South Asian Islam must be grateful.

WAYNE WILCOX
Columbia University

ELIZABETH A. DEXTER of Belmont, Massachusetts, died March 26, 1971, at the age of eighty-four. From 1923 to 1927 Mrs. Dexter taught history at Skidmore College; she was later a tutor in history at Radcliffe College. Her publications include *Colonial Women of Affairs* (1931) and *Career Women of America, 1776-1840* (1951). At the time of her death she was preparing a manuscript on Charlotte, Lady Scott, wife of Sir Walter Scott. Mrs. Dexter had also been associate director of the Unitarian Service Committee during its early years and had engaged in intelligence work in Lisbon for the Office of Strategic Services. At various times she held national and state positions with the American Association of University Women, the League of Women Voters, and the Women's International League for Peace and Freedom.

Our profession in general and medieval studies in particular have suffered a tragic and irreparable loss with the untimely death of ROBERT STUART HOYT, chairman of the department of history at the University of Minnesota. Only fifty-two, in the full tide of his career and with the promise of many productive years ahead, Professor Hoyt succumbed to leukemia on February 24, 1971, after a long and courageous struggle. By a remarkable coincidence, considering the mobility of our times, he died in Minneapolis, where he had been born (May 20, 1918) and where his father was then himself a professor at the university.

Educated at Harvard where he received his A.B. (1940), M.A. (1942), and Ph.D. (1948), Hoyt was a teaching fellow and tutor in history and later assistant dean of the college (1944-45). After a year of military service he began his

professional career at the State University of Iowa, Iowa City, in 1947 and was promoted to associate rank in 1951. He came to the University of Minnesota following the retirement of A. C. Krey in 1955. Two years later he attained the rank of full professor, and in 1966 he was elected to the chairmanship of the department.

Trained in a great tradition, Hoyt was particularly interested in the constitutional history of medieval England, but his knowledge of the whole field of the Middle Ages was encyclopedic. As a consequence his numerous doctoral candidates wrote their dissertations on a variety of subjects. Among his own publications were *The Royal Demesne in English Constitutional History* (1950); his well-known and widely used textbook, *Europe in the Middle Ages* (1957; 2d ed., 1966); and a remarkably useful pamphlet, *Feudal Institutions* (1961). He was also the editor of *Life and Thought in the Middle Ages* (1967), a book incorporating the lectures given by a number of distinguished medievalists who participated in a well-received series sponsored by the University of Minnesota in 1963. In addition to his books Hoyt was the author of articles published in *Speculum*, *English Historical Review*, *Traditio*, and the *Journal of British Studies*; he was also a contributor to the *Medieval Miscellany for D. M. Stenton* (1962) and the *Album Helen Maud Cam* (1963). In 1967 he founded the *International Medieval Bibliography*, a project for which he served as director. In recognition of his scholarship Hoyt was elected a fellow of the Royal Historical Society, and he was president of the Middle West Medieval Conference in 1966-67.

Stuart Hoyt was a fine teacher who stimulated undergraduates and graduates alike and brought medieval studies at the university to a most flourishing condition. Moreover, his administrative talents were extraordinary: his tenure as chairman was exceedingly beneficial to the department, but he was also a pillar of strength in college and university affairs, particularly in the crucial period that began in the spring of 1970. Coauthor of the constitution of the College of Liberal Arts, vice-chairman of the University Senate, and a valuable member of the important Senate Committee on Educational Policy, he fully merited the praise of President Malcolm Moos, who called him a "constructive and creative citizen of the University."

Various plans are under way to honor and perpetuate the memory of this good friend, outstanding teacher, and admirable scholar. The members of his department have established the R. S. Hoyt Memorial Fellowship, to which his friends and admirers elsewhere are invited to contribute.

TOM B. JONES
University of Minnesota

He was seventy-nine, but he was still entranced by the privilege of talking to young minds. He was preparing his lectures for two weeks of the coming spring term at Pennsylvania State University. *Deo volente!* he would write to those who loved him for his gentleness and kindness and consideration of others.

With the death of HANS KOHN in Philadelphia at the University of Pennsylvania Hospital on March 16, 1971, the profession lost one of its most distinguished members and the world intellectual community its outstanding scholar of nationalism. Born in Prague on September 15, 1891, Kohn was reared in an atmosphere of explosive nationalism in the old Austro-Hungarian Empire. The young man who sensed the assertion of Bohemian independence was to devote a long and brilliant career to the study of that most important historical force.

Serving in the Austrian army in World War I, Kohn was captured and sent to a prisoner-of-war camp in Siberia, where he lived through the two Russian Revolutions. After the war he traveled widely in Europe and the Middle East, lived in Paris, London, and Jerusalem, and finally settled in the United States as teacher and scholar.

Kohn's first post on the American scene was in New York City at the New School for Social Research, to which he thereafter retained his loyalty. He taught at Smith College from 1933 to 1949, the last eight years as Sydenham Clark Parsons Professor of History, and at The City College of New York from 1949 until his retirement in 1962. Before and after retirement he lectured at many other institutions, including Harvard, St. Joseph's, Pennsylvania, Buffalo, Notre Dame, Denver, Texas, Chicago, Dartmouth, Northwestern, Douglas, and Ohio Wesleyan. In his active career he was Fulbright Professor at the Free University in Berlin, Guggenheim fellow, member of the Institute

for Advanced Study at Princeton, Fellow for Advanced Studies at Wesleyan, and honorary president of the International Society of the History of Ideas. He contributed many basic articles to encyclopedias, especially the *Encyclopaedia Britannica* and the *Encyclopedia of the Social Sciences*. After 1945 he went to Europe on many governmental missions.

Eclectic in his historical interests, Kohn produced a notable series of publications, mainly in intellectual history. In 1963 Wallace Sokolsky, his devoted friend, compiled a bibliography of forty-two of Hans Kohn's books, many of which were translated into foreign languages throughout the world. Though his chief interest was nationalism, he produced studies of a complementary nature, such as those on the "pan" movements and national character (*The Making of the Modern French Mind* [1955], *The Mind of Modern Russia* [1955], and *The Mind of Germany* [1960]). From his prolific pen came a long series of essays and book reviews for the scholarly journals and other periodicals and newspapers.

Kohn's classic *The Idea of Nationalism* (1944), his major work, set a new standard for the study of that complex phenomenon. Along with Carlton J. H. Hayes of Columbia University he was a pioneer scholar of nationalism. His conception of nationalism as "first and foremost a state of mind, an act of consciousness" was the first expression of its psychological roots. He presented a horizontal conceptualization of nationalism, in which he spoke of a dichotomy between Western and non-Western forms. This analysis was a vivid counterpart to Hayes's vertical formulation stressing the chronological historical evolution of modern nationalism. Both great scholars gave meaning and content to anism filled with inconsistencies, contradictions, and paradoxes.

Throughout his life Kohn was deeply interested in the manifold facets of German history. In his later trips to the Bonn Republic he came away much impressed by the work of the younger German historians. He saw contemporary Germans as far less addicted to a metaphysical *Anschaung* than in the past. Most important of all, he regarded the German mind as now tuned spiritually toward the West and the free world. He believed that the possibility of a free Germany in the Western sense was

far greater now than at any time in the past. In his view this was one of the more encouraging developments of the twentieth century.

Although his native language was German, Kohn's English was literary and grammatically word perfect. This ability recalls that of the Polish novelist Joseph Conrad, who became a master of English style. Kohn's writing was distinguished by grace, elegance, and clarity of expression. His facility at classical allusions revealed the nature of his early training as well as his special talent as scholar and writer.

Somewhat shy and soft-spoken in private life, Kohn underwent instant metamorphosis on the lecture platform or at the classroom desk. Audiences were fascinated by his deep, booming Central European-accented voice, his dramatic gestures, and his ability to express his ideas and principles in clear-cut, beautifully phrased sentences.

Humanist and humanitarian, Kohn did what he could to popularize the free spirit. Humanity was the central focus of his interests. Added to his humanism was a strong humanitarian impulse. His values were always on the side of tolerance, decency, and justice. All who knew him were aware of his compassion for the weak and underprivileged and for his unerring sense of tone, tact, and taste in his interpersonal relations. His enthusiasm and energy were contagious, especially to generations of students who recognized his scholarly integrity, his faith in man as a rational animal, and his optimism in a world of pessimism and doubt.

His students and audiences will long remember the dynamic scholar who dedicated himself to making people "become excited over the right things." His readers will recall the way he linked scholarship with literary grace. His close friends will never forget the gentility, thoughtfulness, and warmth of a rare human being.

LOUIS L. SNYDER

City College,

City University of New York

EUGENIA DICKSON LEJEUNE, former marine librarian and archivist-librarian of the George C. Marshall Research Library in Lexington, Virginia, died on March 12, 1971. Miss Lejeune was in charge of the records section of the Marine Corps School Reference Library in Quantico during World War II. Before joining

the Marshall Foundation staff in 1957 she was called back to active duty four times to assist with the Marine Corps Historical Section and to be a consultant with the Fleet Marine Force Intelligence Section.

ABRAHAM A. NEUMAN, president of Dropsie College for Hebrew and Cognate Learning (now Dropsie University) from 1941 to 1960 and an outstanding authority on the history of the Jews in Spain, died on November 20, 1970, at the age of eighty.

Born in Brezan, Austria, on September 23, 1890, and descended from an old rabbinic family, he was brought to the United States at the age of eight. He was educated in several New York parochial schools and at the Rabbi Isaac Elchanan Yeshivah, and he graduated in 1909, at the age of nineteen, from Columbia University. He subsequently studied at the Jewish Theological Seminary of America, from which he was ordained as rabbi in 1912. In the same year he received his master's degree from Columbia University. Two years later he was awarded the degree of Doctor of Hebrew Letters from the Seminary.

His teaching career began early. In 1912 he became instructor of Jewish history at the Teachers Institute of the Jewish Theological Seminary of America, and he was appointed to the faculty of Dropsie College in 1913. In 1923 he was appointed associate professor and in 1934 full professor, to be followed in 1941 by appointment to the presidency of Dropsie College, a post that he held until 1966.

Between 1919 and 1927 Dr. Neuman served as rabbi of Congregation B'nai Jeshurun in Philadelphia, and from 1927 to 1943 he was rabbi of Mikveh Israel Congregation in Philadelphia, the second oldest congregation in America. Following his resignation as rabbi in 1943 the office of honorary rabbi was created and this title conferred upon him. He was the first American rabbi to hold such a distinction.

As a historian Dr. Neuman made a significant contribution to the development of Jewish historiography when he exploited the rich data preserved in the Jewish legal *responsa* literature as essential sources for reconstructing the history of the Jews in Christian Spain. No previous historian had tapped these sources and demonstrated their crucial value for a multi-

dimensional picture of this facet of Spanish Jewish history. The outcome of his research, a two-volume work entitled *The Jews of Spain: Their Social, Political and Cultural Life during the Middle Ages* (1942), has become something of a classic, receiving a second printing in 1944 and a third in 1948. Notable, too, has been his contribution to the relatively undeveloped field of Jewish historiography: "Josippon, History and Pietism," "Josippon and the Apocrypha," "The *Shevet Yehudah* in 16th Century Historiography," "Samuel Usque, Historian of the 16th Century," and "Medina, Historian of the Inquisition"—all to be found in his *Landmarks and Goals* (1953).

So devoted was Dr. Neuman to the discipline of Jewish history that he continued his teaching throughout the years of his presidency. His basic appreciation and his rich knowledge of the creative past of the Jewish people inspired him to develop Dropsie College into a great nonsectarian center of diversified Judaic and cognate studies. He was responsible for setting up new departments in Jewish philosophy, Hebrew literature, the history of Semitic civilization, comparative religion, education, Assyriology, and, in 1948, contemporary Middle Eastern studies. A most significant project that he was instrumental in launching was the Dropsie College Jewish Apocryphal Literature series. He was also coeditor for many years of the *Jewish Quarterly Review* and participated actively in a wide variety of scholarly undertakings. His accomplishments were recognized

by the many institutions that awarded him honorary degrees. Above all, Dr. Neuman's career reflected the steady maturation of Jewish historiography and Judaic and cognate studies in the United States.

ELLIS RIVKIN

Hebrew Union College,
Cincinnati

RUTH ANNA KETRING NUERMBERGER, widow of Gustave Adolph Nuermberger, died on November 18, 1970. From 1931 to 1943 Dr. Nuermberger was curator of manuscripts in the library of Duke University, where she had received both the A.M. and Ph.D. degrees. In 1960 she became a public affairs officer with the Department of State, in charge of liaison between the foreign press and American historians. Her publications include: *Charles Osborn in the Anti-Slavery Movement* (1937), *The Free Produce Movement: A Quaker Protest against Slavery* (1942), and *The Clays of Alabama: A Planter-Lawyer-Politician Family* (1958).

Other members of the association who have died recently include: Douglass Adair of Claremont, California, a life member of the association since 1937; Hastings Eells, professor emeritus of history at Ohio Wesleyan University; Rev. M. M. Fisher of Richmond, Virginia; Gerrit P. Judd IV, professor of history at Hofstra University; and Robert W. Masters of Ferrum Junior College, Ferrum, Virginia.

Festschriften and Miscellanies

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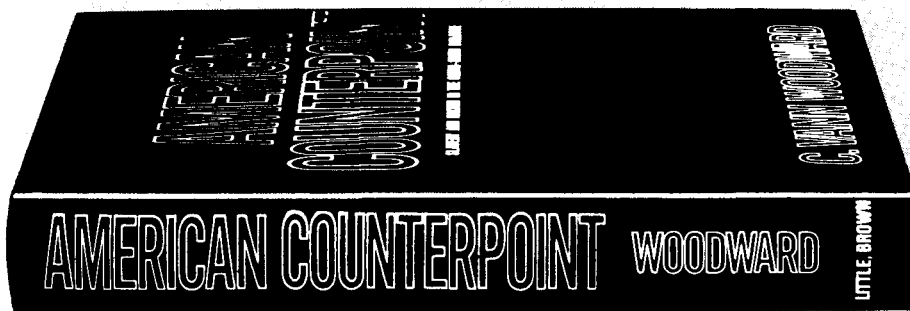
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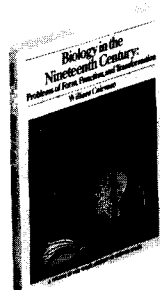
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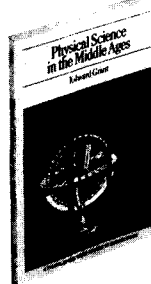


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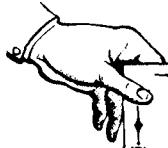
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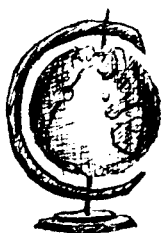
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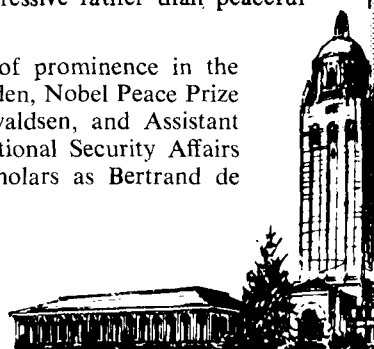
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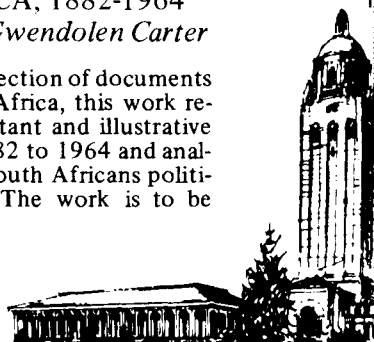
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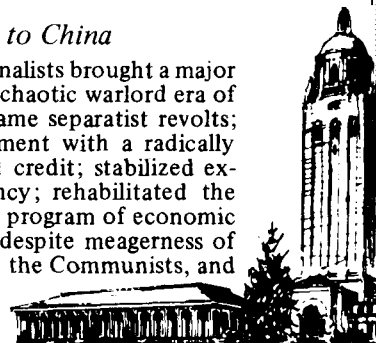
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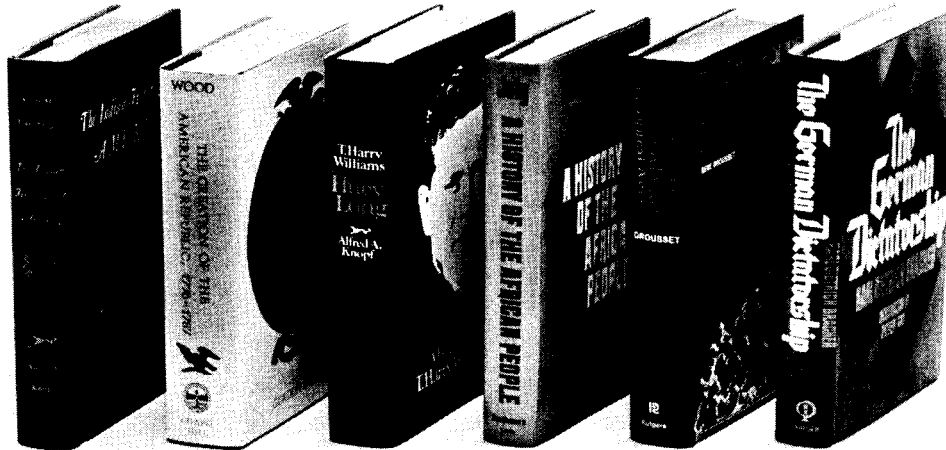
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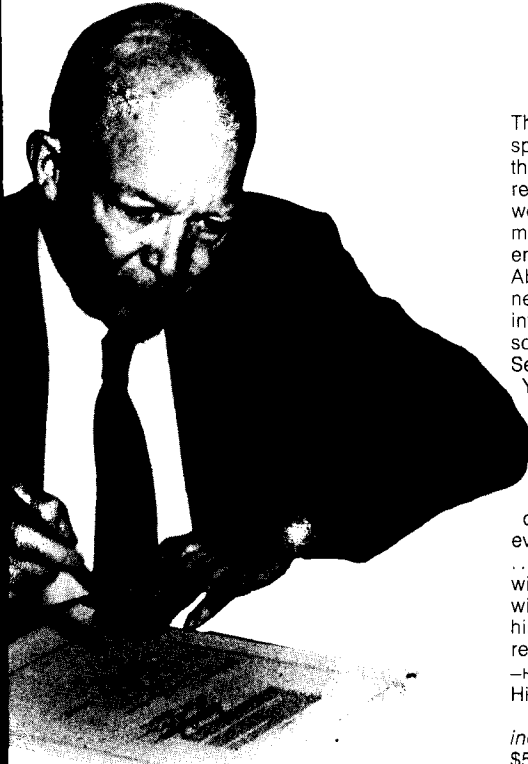
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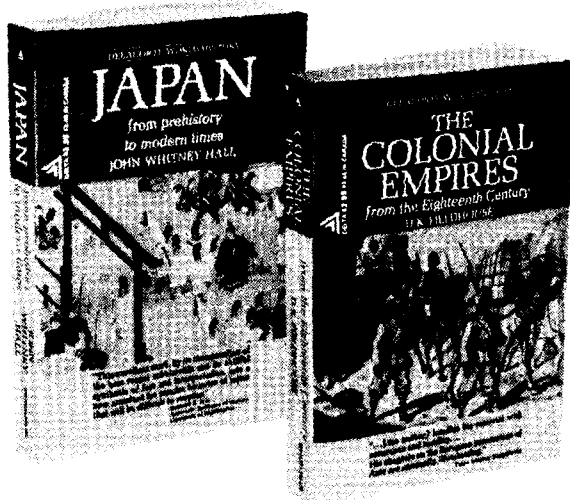
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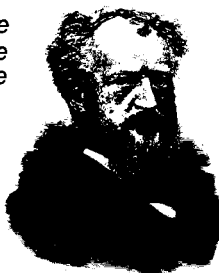
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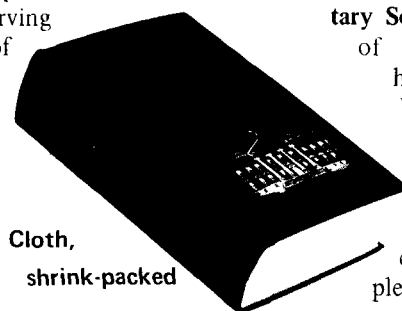
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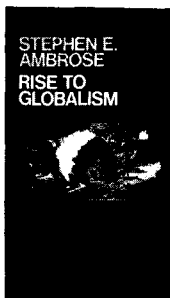
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